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ART. XXI.—*Results of Magnetical Observations made at Eastport Maine, between* 1860 *and* 1864, *for the United States Coast Survey;* communicated by A. D. BACHE, Supdt., under authority from the Treasury Department.

THESE observations were made in connexion with the general system of magnetical determinations on the coast, and with the special object of ascertaining the law of the secular change in the easternmost coast region of the United States.

At the Eastport station, selected for that purpose, the plan of work required the observation of the magnetic declination, dip and horizontal intensity, during three days near the middle of each month. It is worthy of remark, as a result of these observations, continued for over four years, that the most important magnetic features of a locality may be developed by a system of observations requiring comparatively but a small sacrifice of time, such as is in the power of many persons to make, who are engaged in other pursuits.

The instruments used were a theodolite magnetometer and a dip circle, both constructed by Mr. Wm. Wurdemann, according to designs by Mr. J. E. Hilgard.

The principal novelty in the theodolite magnetometer, besides many details affording greater convenience in making the adjustments than heretofore had, consists in having the collimator magnets so light as to be readily supported by a single silk fiber, the torsion resistance of which is extremely small, and susceptible of a stable adjustment. The magnets are $3\frac{1}{2}$ inches

long, $\frac{5}{16}$ inch external and $\frac{4}{16}$ internal diameter; value of a scale division 15″. The dip-circle has a diameter of 5½ inches, and reads to 30″ by means of two verniers; the needles are 9½ inches long, and the pointings are made with microscopes attached to the vernier-arms, on small holes pierced through the needles. Those used during the first half of the series have axles and pivots of the ordinary construction, but from October 1862 two needles were used having their axles so fitted in arbors, as to admit of being turned about their centers, by which means they may be brought to rest on different parts of the pivots. The observations were made on each day in three different positions of the pivots, and the errors arising from faults in their figure appear to be very nearly eliminated.

The position of the magnetic observatory is on the parade ground of Fort Sullivan near Eastport, in lat. 44° 54′·4, long. 66° 58′·9, west of Greenwich.

The observations were made successively by Messrs. G. B. Vose, S. Walker, E. Goodfellow, A. T. Mosman and H. W. Richardson, all attached to the Coast Survey.

The discussion herewith presented of the observations, has been made by Assistant C. A. Schott. The results are stated under the several heads of declination, dip and horizontal intensity.

1. *Declination.*—The zero of the collimator magnet, or the position of the magnetic axis on its scale, was determined by inversions each month, and its readings on the circle referred to a distant mark of known azimuth. On four days about the middle of each month the declination readings were recorded every half hour, between the morning minimum and the afternoon maximum, generally between the hours of 6 A. M. and 2 P. M. Each monthly declination result is therefore the mean from observations made on four days.

To obtain the mean declination of the day or that value which would result from 24 hourly observations, a small correction is applied, derived from the discussion of the Girard College series, which shows that the mean of the least and greatest declination of the day gives the *west* declination too large, as does also the mean of the half hourly readings between those extremes, the amount of excess being the same. We have accordingly the following corrections to our means *expressed in parts of the diurnal range:*

In January,	$-\frac{1}{10}$	In July,	-0
" February,	$-\frac{1}{15}$	" August,	-0
" March,	$-\frac{1}{20}$	" September,	$-\frac{1}{25}$
" April,	$-\frac{1}{20}$	" October,	$-\frac{1}{10}$
" May,	-0	" November,	$-\frac{1}{10}$
" June,	-0	" December,	$-\frac{1}{8}$

Diurnal range of the declination.—The difference between the maximum and minimum value of the declination set down in the table for each month, is the mean of four days of observations.

	1860.	1861.	1862.	1863.	1864.	Means of 5 years.	Observed annual inequality.	Cor. for 11 years. inequality.	Corrected annual inequality.
Jan.,	15·2	8·6	12·1	10·6	11·4	11·6	−2·1	−0·4	−2·5
Feb.,	12·6	11·7	11·0	8·0	9·0	10·5	−3·2	−0·3	−3·5
March,	16·4	18·1	13·1	11·8	13·0	14·5	+0·8	−0·2	+0·6
April,	16·0	13·6	18·4	16·6	11·6	15·2	+1·5	−0·1	+1·4
May,	18·7	8·8	14·2	15·6	15·9	14·6	+0·9	0·0	+0·9
June,	18·1	12·3	17·7	15·5	13·2	15·4	+1·7	0·0	+1·7
July,	13·0	16·3	17·8	14·6	13·6	15·1	+1·4	0·0	+1·4
Aug.,	21·5	18·7	19·8	14·5	...	18·3	+4·6	0·0	+4·6
Sept.,	19·5	18·8	17·4	15·5	...	17·5	+3·8	+0·1	+3·9
Oct.,	18·7	10·4	14·1	14·2	...	14·0	+0·3	+0·2	+0·5
Nov.,	13·7	9·2	8·9	8·9	...	9·9	−3·8	+0·3	−3·5
Dec.,	7·8	9·0	8·0	6·4	...	7·5	−6·2	+0·4	−5·8
Annual Means,	15·9	13·0	14·4	12·7	...	13·7			

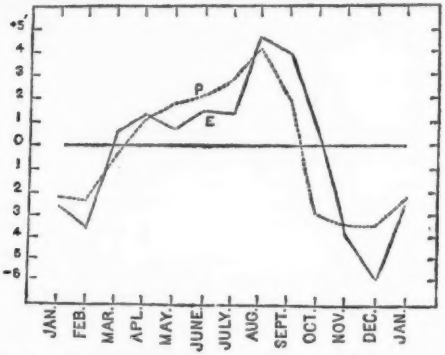
The average difference between the 7 values of 1864 and the mean of the 4 years preceding is −1′·6. Applying this to the 4 years means for the remaining five months, the interpolated means for 1864 become, for Aug. 17′·0, for Sept. 16′·2, for Oct. 12′·8, for Nov. 8′·6, and for Dec. 6′·2. The column headed "means of 5 years" is completed with the aid of these values. The interpolated mean for 1864 is 12′·4. The average annual change is − 0′·9.

The 11 year inequality in the range of the diurnal movement appears quite plainly in the annual means. The year 1860 was one of maximum and 1866 of minimum, according to the observations of the solar spots; in 1865 the average range will, therefore, be a little above 12′, and in 1866 a little below this value, giving a range of variability due to the 11 years inequality of nearly 4′. The corresponding quantity at Philadelphia is nearly 2′ from observations between 1840 and 1845.

Our series of observations extends over less than one half of the eleven year period, a correction has therefore been applied to obtain the annual inequality in the diurnal range free from the eleven year period as shown in last column of above table.

The annual inequality of the diurnal range at Eastport and Philadelphia compare as shown by the annexed diagram, the full line being for Eastport, the broken line for Philadelphia. The Toronto curve also agrees well with these curves.

ANNUAL INEQUALITY IN THE DIURNAL RANGE.



The diurnal range reaches a maximum in August and a minimum in December. There is reason to suppose that the curve is a compound one, consisting of two waves, changing its character according to changes in epochs and amount of these component systems.

Epochs of greatest diurnal deflection.—The average epochs of the morning east elongation and the afternoon west elongation are given in the following table:

	East Elongation. H.	M.	West Elongation. H.	M.		East Elongation. H.	M.	West Elongation. H.	M.
Jan.,	8	30	1	20	July,	7	20	1	00
Feb.,	8	40	1	50	Aug.,	7	10	0	20
Mar.,	8	20	1	10	Sept.,	7	30	0	50
Apr.,	7	50	1	10	Oct.,	7	50	1	10
May,	7	10	0	20	Nov.,	8	00	1	00
June,	6	50	0	40	Dec.,	9	00	1	00

For the summer half year from April to September included, the morning east elongation occurs at $7^h\ 20^m$, and for the winter half year from October to March included, the east elongation occurs at $8^h\ 20^m$; at Philadelphia these epochs were $7^h\ 33^m$ and $8^h\ 24^m$ respectively. For the summer half year the afternoon west elongation occurs at $0^h\ 40^m$, and for the winter half year at $1^h\ 20^m$. At Philadelphia these epochs were $1^h\ 8^m$ and $1^h\ 25^m$ respectively. On the average for the year, the turning epochs are $7^h\ 50^m$ A. M., and $1^h\ 0^m$ P. M.

Mean monthly values of the declination, observed at Eastport between August 1860 and July 1864.

These values were obtained as follows: Let $D_{,}$ = mean of daily minimum and maximum declination, $D_{,,}$ = mean of all half hourly declinations, between these extremes and including them, then $D = \frac{D_{,}+D_{,,}}{2} + C$, where C = correction to refer the

declination to its average value of the day. The minutes given in the table are to be added to 17°.

	1860, 1861.	1861, 1862.	1862, 1863.	1863, 1864.	Means.
	′	′	′	′	′
Aug.,	58·1	59·5	60·1	64·2	60·5
Sept.,	56·9	61·1	61·1	63·6	60·7
Oct.,	57·2	60·1	60·8	63·7	60·5
Nov.,	59·8	61·7	63·5	64·3	62·3
Dec.,	58·5	59·2	62·3	62·7	60·6
Jan.,	56·5	61·0	62·1	62·8	60·6
Feb.,	58·1	60·6	60·5	62·5	60·4
March,	58·1	58·4	61·7	63·4	60·4
April,	58·7	61·2	62·0	63·0	61·2
May,	58·0	60·1	61·1	61·6	60·2
June,	61·0	59·3	60·7	61·2	60·5
July,	58·1	59·2	60·7	62·0	60·0
Means,	58·2	60·1	61·4	62·9	60·65

The average value for the period is 18° 00′·65.

Annual effect of the secular change.—We deduce the annual effect of the secular change directly from the preceding table.

Annual increase of declination between			1861 and 1862,	1′·9
"	"	"	1862 " 1863,	1·3
"	"	"	1863 " 1864,	1·5
Average annual increase of west declination,				1·6

which, considering the locality, appears a remarkably small value. According to our previous information we might have expected an annual increase of about 4′. Either the above small result indicates a local deviation from the general law, or else at this most *easterly* station we are approaching the period of stationary condition which, from previous researches, may be expected to take place before the close of the present century.

In July 1865 the declination was again observed in order to obtain a confirmation of its small annual increase; from four days of observation 18° 04′·7 was found, and since the annual mean is found by adding 1′·4 (vide previous years) the declination for 1865 becomes 18° 06′·1, and the annual increase apparently equals 2′·4.

The annual mean declination corrected for imperfect number of observations in 1860 and 1864, is as follows:

In 1860,	17° 57′·1 W.	In 1863,	18° 02′·3 W.
" 1861,	17° 59′·2 "	" 1864,	18° 03′·7 "
" 1862,	18° 00′·6 "	" 1865,	18° 06′·1 "

Annual inequality of the declination.—The difficulty of establishing this inequality experimentally is well known, and long continued and frequent observations have failed to furnish a satisfactory general elucidation of this subject, in reference to which the Coast Survey Report for 1860, pp. 311–12, may be consulted.

The values for Eastport have been derived as follows:

	17°+	Correct'ns for secular change.	Corrected declination.	Annual inequality.
Aug.,	60·5	+0·7	61·2	+0·55
Sept.,	60·7	+0·6	61·3	+0·65
Oct.,	60·5	+0·5	61·0	+0·35
Nov.,	62·3	+0·3	62·6	+1·95
Dec.,	60·6	+0·2	60·8	+0·15
Jan.,	60·6	+0·1	60·7	+0·05
Feb.,	60·4	−0·1	60·3	−0·35
March,	60·4	−0·2	60·2	−0·45
April,	61·2	−0·3	60·9	+0·25
May,	60·2	−0·5	59·7	−0·95
June,	60·5	−0·6	59·9	−0·75
July,	60·0	−0·7	59·3	−1·35

The following comparative table contains the annual inequality for Eastport, Philadelphia and Toronto, the latter for three different epochs, the last two of which are derived from Mr. Kingston's paper on "Monthly absolute values of the magnetic elements at Toronto, from 1856 to 1864 inclusive:"

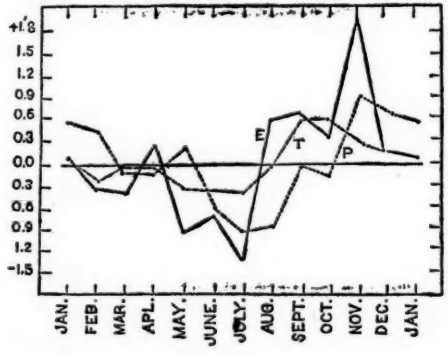
Annual inequality of the Magnetic Declination. + indicates west deflection, − east deflection from Normal declination.

	Eastport, 4 years, 1860-64.	Phila. 5 years, 1840-45.	Toronto. 7 years. 1845-51.	Toronto. 4 years, 1856-59.	Toronto. 5 years, 1860-64.	Toronto. 16 years.
Jan.,	+0·05	+0·5	+0·1	−0·2	+0·10	+0·03
Feb.,	−0·35	+0·4	−0·5	+0·2	−0·35	−0·28
March,	−0·45	−0·1	−0·2	+0·5	−0·14	0·00
April,	+0·25	−0·1	0·0	+0·1	−0·31	−0·07
May,	−0·95	+0·2	−0·1	−0·4	−0·67	−0·35
June,	−0·75	−0·6	−0·5	−0·7	+0·03	−0·38
July,	−1·35	−1·0	−0·8	−0·5	+0·30	−0·38
Aug.,	+0·55	−0·9	−0·2	−0·1	+0·19	−0·06
Sept.,	+0·65	0·0	+0·7	+0·7	+0·13	+0·52
Oct.,	+0·35	−0·2	+1·0	0·0	+0·40	+0·56
Nov.,	+1·95	+0·9	+0·3	+0·1	+0·41	+0·29
Dec.,	+0·15	+0·7	+0·3	+0·1	−0·07	+0·14

The general agreement of the Eastport, Philadelphia, and the approximate resemblance of the Toronto (16 year) curves is shown on the annexed diagram; the annual range keeps probably within 2′, and it is not clear whether the annual inequality is subject to a variation of a comparatively short period, a question which remains to be cleared up by future observations. The effect of the annual inequality is to diminish the west declination in July and to increase it in November, these being the months when it reaches its greatest amount.

2. *Dip.*—The series of observations and results extends from January 1860, to July 1864, during which period the instruments and observers were changed several times. A partial discussion of the observations of 1860–61–62 showed an annual

diminution of the dip of 2′·2 in the first year, and of 3′·0 in the second year. These evidences of a *decreasing* secular change



appeared at that time anomalous, but it will be seen that they are borne out by subsequent observations, and likewise in other places. The observations of the dip at Washington in 1860 first *indicated* a change of sign in the secular effect, which fact is now fully established by later observations.

The values given in the following table for each month are the means of observations made on three, and sometimes on four days with two needles; the polarity of the needles was reversed during each set:

Monthly means of magnetic dip.

	1860.	1861.	1862.	1863.	1864.
	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″
Jan.,	75 51·6	75 52·9	75 50·2	75 47·0	75 46·1
Feb.,	52·7	53·4	49·5	45·6	46·7
March,	54·5	52·7	49·0	47·4	46·7
April,	54·4	52·1	49·3	49·0	44·6
May,	53·5	49·5	48·1	47·9	46·0
June,	51·8	49·8	48·0	48·6	45·6
July,	52·9	49·9	48·4	49·1	44·9
Aug.,	54·7	50·4	48·2	50·6	
Sept.,	53·5	51·2	48·3	49·8	
Oct.,	53·3	50·9	49·2	49·5	
Nov.,	51·9	49·9	47·4	48·3	
Dec.,	52·3	49·3	46·9	46·8	
Means,	75 53·1	75 51·0	75 48·5	75 48·3	

In July 1865 the dip was found from observations on four days, 75° 44′·7, which, when reduced to the mean of the year, gives 75° 44′·8 for 1865.

Annual effect of the secular change.

1861—1860,	−2·1
1862—1861,	−2·5
1863—1862,	−0·2
1864—1863,	−2·0
Mean.	−1·7

On the average, therefore, the annual diminution is very nearly $1'\frac{3}{4}$.

Annual inequality of the dip.—If we take the monthly means for the four years 1860–1863, correct them for secular change, the monthly effect of which is −0′·14, and then take the differences of each value from the general mean, we shall have the annual inequality, as given in the following table, in which for comparison is likewise given the annual inequality for Toronto, derived by the same treatment from the results given by Mr. Kingston. The annual effect of the secular change for Toronto between 1860 and 1864 is −0′·90, about half that of Eastport.

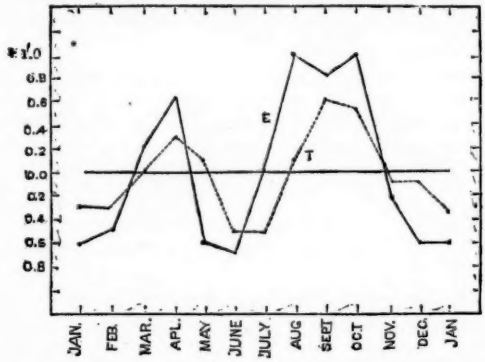
A + sign indicates greater dip, a − sign less dip than the normal value.

	Observed means.	Corrections for secular change.	Corrected dip.	Annual inequality.	
				Eastport.	Toronto.
	° ′	′	° ′	′	′
Jan.,	75 50·4	−0·77	75 49·6	−0·6	−0·3
Feb.,	50·3	−0·63	49 7	−0·5	−0·3
March,	50·9	−0·49	50·4	+0 2	+0·1
April,	51·2	−0·35	50·8	+0·6	+0·3
May,	49·8	−0·21	49·6	−0 6	+0·1
June,	49·6	−0·07	49·5	−0·7	−0·5
July,	50·1	+0·07	50·2	0·0	−0·5
Aug.,	51·0	+0·21	51·2	+1·0	+0·1
Sept.,	50·7	+0·35	51·0	+0·8	+0·6
Oct.,	50·7	+0·49	51·2	+1·0	+0 5
Nov.,	49·4	+0·63	50·0	−0·2	−0·1
Dec.,	48·8	+0·77	49·6	−0·6	−0 1
Mean,	75 50·2				

The law for the two stations is evidently the same as shown by the annexed diagram. The range of the inequality at Toronto is less than at Eastport, where it hardly reaches +1′ and −1′. The dip is greater about the equinoxes and less about the solstices, also greatest at the autumnal equinox and least at the winter solstice.

3. *Horizontal intensity.*—The observations for horizontal intensity were made by vibrations and deflections, with the theodolite magnetometer, on four days near the middle of each month. The moment of inertia and temperature coefficient of the magnet employed were ascertained by numerous experiments. In order to convey an idea of the accuracy of the

observations, the values of the magnetic moment of the magnet are subjoined as resulting from the monthly determinations dur-



ing the last two years when its magnetic condition had become nearly constant.

Magnetic moment of magnet A *at 62° Fahr.*

1862.	July,	0·4017	1863,	0·4004
	Aug.,	·4015		·3999
	Sept.,	·4013		·4003
	Oct.,	·4016		·4003
	Nov.,	·4012		·4003
	Dec.,	·4009		·4000
1863.	Jan.,	·4007	1864,	·4002
	Feb.,	·4006		·4001
	Mar.,	·4007		·4003
	Apr.,	·4008		·4003
	May,	·4008		·4007
	June,	·4006		·4001

Table of observed values of the horizontal force.

	1860.	1861.	1862.	1863.	1864.	Mean 1860-64, 5 years.
Jan.,	3·298	3·306	3·297	3·304	3·308	3·303
Feb.,	3·299	3·311	3·300	3·304	3·308	3·304
March,	3·300	3·308	3·302	3·307	3·311	3·306
April,	3·311	3·307	3·302	3·314	3·313	3·309
May,	3·309	3·315	3·307	3·318	3·315	3·313
June,	3·313	3·316	3·309	3·314	3·320	3·314
July,	3·315	3·316	3·305	3·313	3·320	3·314
Aug.,	3·306	3·305	3·306	3·311	....	3·308
Sept.,	3·307	3·308	3·304	3·308	....	3·308
Oct.,	3·307	3·297	3·303	3·306	....	3·304
Nov.,	3·308	3·297	3·304	3·308	....	3·305
Dec.,	3·309	3·297	3·302	3·308	....	3·305
Mean,	3·307	3·307	3·303	3·310		3·308

In computing the last column the wanting values for 1864 have been supplied by interpolation. In July 1865, from observations on 4 days the horizontal force was found 3·319; referring this to the annual mean we subtract 0·006 and obtain for 1865 the value 3·313.

Secular change of the horizontal force.—Examining the annual means we notice at first a diminution of the force till the beginning of the year 1862; the force after this date shows an annual increase of 0·0012 parts of the force. In a previous discussion (Coast Survey Report 1861, Appendix No. 22) the horizontal force along the Atlantic coast was found to diminish annually 0·0011 parts of the force. This diminution, according to the Eastport observations, has now ceased, and changed to an increase. This reversal of the change corresponds to the observed diminution of the dip, and is supported by the Toronto observations, where it took place, according to G. T. Kingston, Director of the Observatory, in 1860, which is the year of minimum force. At Toronto the present annual increase amounts to 0 0010 parts of the force.

We have further from the Eastport observations the following table of the total force, F=H sec I.

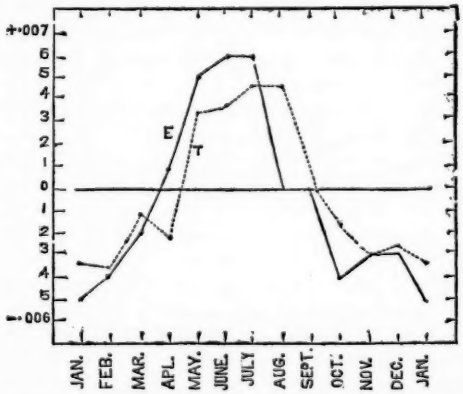
	I.	H.	F.
1860,	75° 53′·1	3·307	13·56
1861,	75° 51′·0	3·307	13·53
1862,	75° 48′·5	3·303	13·47
1863,	75° 48′·3	3·310	13·50
1864,	75° 46′·3	3·313	13·48

The total force appears to be decreasing in conformity with the diminution of the dip; at Toronto it has likewise decreased between 1845 and 1864. From observations at Key West, Florida, it appears that both the horizontal and total forces were diminishing between 1860–1864.

Annual inequality of the horizontal force.—The annual variation can be found directly by subtracting the annual mean from each monthly result, since the minimum year is nearly midway between the extreme epochs. At Toronto, likewise, the minimum year is midway between the extreme years; the series there extends over nine years, between 1856 and 1864.

Annual inequality of the horizontal force, at Eastport 1860–1864 (4½ *years*); *at Toronto* 1856–1864 (9 *years*).

	H at Eastport.	Annual inequality, Eastport.	H at Toronto.	Annual inequality, Toronto.
Jan.,	3·303	−·005	3·4850	−·0034
Feb.,	·304	−·004	·4847	−·0037
March,	·306	−·002	·4873	−·0011
April,	·309	+·001	·4863	−·0021
May,	·313	+·005	·4918	+·0034
June,	·314	+·006	·4921	+·0037
July,	·314	+·006	·4931	+·0047
Aug.,	·308	·000	·4930	+·0046
Sept.,	·308	·000	·4889	+·0005
Oct.,	·304	−·004	·4868	−·0016
Nov.,	·305	−·003	·4855	−·0029
Dec.,	·305	−·003	·4858	−·0026
Means,	3·308		3·4884	



The agreement in the annual inequality at the two places is as close as can possibly be expected. For stricter comparison it would be necessary to convert the tabular numbers of the inequality into parts of the respective forces.

ART. XXII.—*On the Age of the Coal Formation of China;* by Dr. J. S. NEWBERRY: addressed to RAPHAEL PUMPELLY, Esq.

THE fossil plants you were kind enough to submit to me for examination, though few in number and somewhat fragmentary, have proved to be of very special interest since they supply the necessary data for determining approximately the age of the strata from which they were taken, and rather unexpectedly prove a large part of the great coal-fields of China to be of Mesozoic age.

This conclusion is based on the entire absence of Carboniferous plants from the collection; and the presence of well marked Cycads—species of *Podozamites* and *Pterozamites*—closely allied to, if not identical with, some heretofore found in Europe and America.

I give below such descriptions of the several species contained in the collection as could be framed from the somewhat meager material submitted to me. Future observations made upon a larger number of more perfect specimens will be necessary before questions of specific identity or difference can be definitively settled, but it is scarcely probable that any facts or specimens hereafter to be obtained will require a modification of the view, that the coal basins which you visited are all Mesozoic, and not Carboniferous.

We have, of course, no right to assume from the interesting facts your explorations have brought to light, that no Carboniferous coal exists in China, for it may very well happen that, as in our own country, coal seams of economical value, but of different ages, will be found there at points not greatly removed from each other; but geologists will not fail to be deeply interested in the fact that so large portions of the coal basins of China, including beds of both anthracite and bituminous coal, worked for hundreds of years—probably the oldest coal mines in the world—are wholly excluded from the Carboniferous formation. So large is this coal-bearing area, indeed, that when joined to the Triassic, Cretaceous and Tertiary coals of North America, they quite overshadow the Carboniferous coals of Europe and the Mississippi valley, and suggest the question whether the name given to the formation, which includes the most important European coal strata has not been somewhat hastily chosen.

Another interesting feature in the fossil plants under consideration is the reappearance, at the far distant points from whence they come, of genera so well known in European and American geology, and the entire absence of the species of *Phyllotheca, Glossopteris,* etc., which have made the Indian and Australian coal floras so puzzling to the paleontologist. There are fragments of a new generic form, probably a Cycad, in the collection, and some obscure specimens that may represent other plants new to science, but the *Pecopteris, Sphenopteris, Podozamites, Pterozamites,* &c., have a very familiar look; and in their resemblance to well known forms give fresh evidence of the monotony of the vegetation of the globe previous to the introduction of the angiospermous forests of the Cretaceous period.

Whether the strata which have furnished these plants should be considered Triassic or Jurassic remains to be determined by future observations, as the fossils yet obtained can hardly be considered sufficient for the solution of that question.

From the "Kwii basin" we have numerous pinnæ of a species of *Podozamites*, undistinguishable from one found by Prof. Emmons in North Carolina in strata now generally regarded as *Triassic;* but associated with these are a few pinnæ of different form, much more elongated and acute, scarcely differing from those of a European Jurassic species (*P. lancolotus* Lind.). Still, the evidence of identity is much stronger in regard to the former species than the latter.

From Piyunsz we have a fine *Pecopteris* with the falcate pinnules so characteristic of the Mesozoic species, and, indeed, very accurately copying the form of *P. Whitbiensis*, a European Jurassic species; but unfortunately the strata which contain this fossil have been much metamorphosed, the coal converted to anthracite, and the nervation of the fern has been entirely obliterated, while the outline remains distinct.

Probably it will be found as difficult—or rather as impossible—in China, as it has proved in this country, to identify all the subdivisions of the Mesozoic strata discernible in Europe. Yet we shall doubtless gather there new proofs of the constancy of the order of sequence in geological history, and new evidence of the stability of the foundations on which geology as a science rests.

I have under my eye, as I write this letter, four collections of fossil plants, which, though from very widely separated localities, are curiously linked together. They are,—

1st. Fossil plants,—Cycads and Conifers,—collected by myself from the "Gypsum Formation" (Triassic) at Abiguiu, New Mexico. Of this collection the most conspicuous and interesting plant is *Otozamites Macombii* N.

2d. A collection of fossil plants—Cycads and ferns—received through Prof. J. D. Whitney from Sonora, Mexico, where they occur with coal strata and Triassic mollusks.

In this collection *Otozamites Macombii* is associated with *Strangerites magnifolia* Rogers, *Pecopteris falcatus* Emm., and other plants occurring abundantly in North Carolina.

3d. A collection of fossil plants—Cycads and ferns—from N. Carolina and Virginia, including, besides the last two mentioned, and many which are new, several species apparently identical with European Triassic plants, of the genera *Haidingera*, *Gutbieria*, *Laccopteris*, &c.; and among other Cycads, *Podozamites Emmonsii* N.

4th. The collection made by yourself in China—Cycads and ferns—in which one of the most distinctly marked plants is *P. Emmonsii.*

In regard to the American localities cited above, there is perhaps no good reason for our withholding assent to the conclusion that the rocks furnishing the fossil plants, are Triassic, but

when we remember how much difference of opinion there has been, and indeed still is, upon this subject, even in the light of large collections of fossils, we can hardly with propriety offer even a conjecture as to the *precise* age of the Chinese coal strata.

To recapitulate. One species of *Podozamites* contained in the collection is apparently identical with an American Triassic species; the other more resembles a European Jurassic plant. The *Pterozamites* resembles both Triassic and Jurassic species, but is identical with neither. The *Pecopteris* has certainly a remarkable likeness to *P. Whitbiensis*, which occurs both in the Liassic and Oolitic floras; and it is not yet certain that it is not also found in the Carolina and Richmond coal basins.

The *Sphenopteris* and *Hymenophyllites* are altogether new, and suggest no affinites of value in this connection—while the *Taxites*, *Equisetites*, &c., are too obscure to afford us any help.

Cleveland, Ohio, Sept. 25, 1865.

ART. XXIII.—*A Second Method of correcting Monthly Means for the unequal length of the Months;* by ERASTUS L. DEFOREST.

IN the May number of this Journal I gave a system of twelve equations for finding the mean temperature, rain-fall, &c., for any *mean month* in terms of the means for the three nearest calendar months. This was done on the supposition that the curve of daily temperatures for any three consecutive months may be represented by a portion of a parabola whose equation is

$$y=a+bx+cx^2.$$

A similar system of equations may be found by assuming that the curve is of the form

$$y=a_0+a_1 \sin (x-e_1),$$

which may be written

$$y=\mathrm{A}+\mathrm{B}\cos x+\mathrm{C}\sin x.$$

Let n_1, n_2, n_3, x_1, and c, retain the same significations which they had in my former article, only instead of denoting days, let them denote the proportional arcs, the days being reduced to arc in the ratio of $365\frac{1}{4}$ days to 360°. By a process of integration similar to that followed before, we shall find that

$$m_1=\mathrm{A}+\frac{1}{n_1}\mathrm{B}[\sin(\tfrac{1}{2}n_2+n_1)-\sin\tfrac{1}{2}n_2]-\frac{1}{n_1}\mathrm{C}[\cos\tfrac{1}{2}n_2-\cos(\tfrac{1}{2}n_2+n_1)],$$

$$m_2=\mathrm{A}+\frac{2}{n_2}\mathrm{B}\sin\tfrac{1}{2}n_2,$$

$$m_3 = A + \frac{1}{n_3} B[\sin(\tfrac{1}{2}n_2 + n_3) - \sin \tfrac{1}{2}n_2] + \frac{1}{n_3} C[\cos\tfrac{1}{2}n_2 - \cos(\tfrac{1}{2}n_2 + n_3)],$$

$$M_2 = A + \frac{2}{c} B \cos x_1 \sin \tfrac{1}{2}c + \frac{2}{c} C \sin x_1 \sin \tfrac{1}{2}c.$$

Let the above be written for brevity

$$\begin{aligned} m_1 &= A + Bb_1 + Cc_1, \\ m_2 &= A + Bb_2, \\ m_3 &= A + Bb_3 + Cc_3, \\ M_2 &= A + Bb'' + Cc''. \end{aligned}$$

Eliminating A, B and C, and employing K and L as auxiliary letters, we shall find this expression for M_2:

$$K = \frac{c''(b_2 - b_3) - c_3(b_2 - b'')}{c_3(b_2 - b_1) - c_1(b_2 - b_3)},$$

$$L = \frac{c_1(b_2 - b'') - c''(b_2 - b_1)}{c_3(b_2 - b_1) - c_1(b_2 - b_3)},$$

$$M_2 = m_2 + (K + L)m_2 - Km_1 - Lm_3.$$

We may now proceed to compute for each of the twelve months separately, the numerical values of b_1, b_2, b_3, c_1, c_3, b'' and c'', and from them the numerical coëfficients K and L, with the following result:

$$\begin{aligned}
M_1 &= m_1 + \cdot0036\, m_1 + \cdot0031\, m_{12} - \cdot0067\, m_2 \\
M_2 &= m_2 - \cdot0127\, m_2 - \cdot0030\, m_1 + \cdot0157\, m_3 \\
M_3 &= m_3 + \cdot0028\, m_3 - \cdot0248\, m_2 + \cdot0220\, m_4 \\
M_4 &= m_4 - \cdot0042\, m_4 - \cdot0199\, m_3 + \cdot0241\, m_5 \\
M_5 &= m_5 + \cdot0016\, m_5 - \cdot0217\, m_4 + \cdot0201\, m_6 \\
M_6 &= m_6 - \cdot0039\, m_6 - \cdot0179\, m_5 + \cdot0218\, m_7 \\
M_7 &= m_7 + \cdot0025\, m_7 - \cdot0199\, m_6 + \cdot0174\, m_8 \\
M_8 &= m_8 + \cdot0025\, m_8 - \cdot0103\, m_7 + \cdot0078\, m_9 \\
M_9 &= m_9 - \cdot0027\, m_9 - \cdot0067\, m_8 + \cdot0094\, m_{10} \\
M_{10} &= m_{10} + \cdot0030\, m_{10} - \cdot0085\, m_9 + \cdot0055\, m_{11} \\
M_{11} &= m_{11} - \cdot0026\, m_{11} - \cdot0046\, m_{10} + \cdot0072\, m_{12} \\
M_{12} &= m_{12} + \cdot0032\, m_{12} - \cdot0064\, m_{11} + \cdot0032\, m_1
\end{aligned}$$

A comparison of this system of equations, found by using a trigonometrical curve, with the former system which was found by using an algebraic curve, is interesting as showing how far the values of the numerical coëfficients are independent of the nature of the curve employed. The fact that these two sets of coëfficients differ but little, tends to establish the general accuracy of both methods, and gives a high degree of probability to the results derived from them. These results are almost identical. For the climate of St. Paul, where the great range of temperature will of course magnify any small discrepancies, the mean temperatures for mean months are by the first method of reduction

13·74	46·92	73·44	46·97
17·79	59·41	69·93	31·50
32·05	68·73	58·68	16·79

and by the second method they are

13·74	46·94	73·44	46·97
17·80	59·43	69·93	31·49
32·08	68·74	58·67	16·79

showing a maximum difference of only ·03.

The equation of the curve of daily temperatures throughout the year is by the first method

$$y=44{\cdot}66+29{\cdot}86 \sin (x-104^\circ\ 41')+ \&c.,$$

and by the second method it is

$$y=44{\cdot}67+29{\cdot}86 \sin (x-104^\circ\ 40')+ \&c.$$

If it is necessary to choose between the two systems, perhaps the second may be preferred for reducing temperatures, because the trigonometrical curve admits points of inflexion, which the parabola cannot have; so that the latter can hardly be said to represent very well the curve of temperatures in the spring and autumn months, where it changes from convex to concave or the reverse. Accordingly we see that for St. Paul the differences between the monthly means found by the two methods are greatest in the spring and autumn months, and null at midsummer and midwinter.

May, 1866.

ART. XXIV.—*On a New Process of Organic Elementary Analysis for Substances containing Chlorine;* by C. M. WARREN.

ORGANIC bodies containing chlorine—and probably those also, that contain bromine and iodine—may be analyzed by a process analogous to that which I have already described for substances containing sulphur.[1]

As in that process, so also in this, the substance is burnt in a stream of oxygen gas, in the manner described in my first paper, on organic Elementary Analysis.[2]

Similarly, also, as in the analysis of sulphur compounds, the chlorine is absorbed and retained during the combustion, by a suitable substance placed in the anterior end of the combustion tube; this substance being subsequently removed, and the chlorine determined therefrom in the usual manner. The carbon and hydrogen, in either process, are determined from the same por-

[1] Proceedings of the American Academy, March, 1865; this Journal, Jan., 1866, xli, 40.

[2] Proceedings of the American Acad., 1864, p. 251; this Journal, xxxviii, 387.

tion of the substance as the sulphur or chlorine, in a manner similar in other respects to that described for simple hydrocarbons.[3]

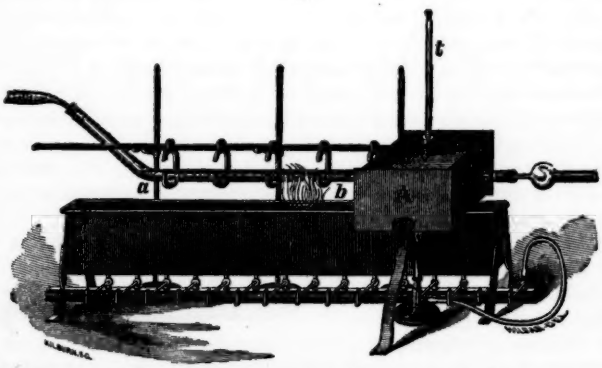
In pursuing this research some difficulty was experienced, as was anticipated, in finding a substance which would absorb and retain the whole of the chlorine, under conditions that would at the same time insure that every trace of the carbonic acid and water should pass through unabsorbed.

The search for this substance was confined to the oxyds of the heavy metals, as these alone, from their strong affinity for chlorine, and weak affinity for carbonic acid, seemed to give encouragement of success.

The difficulty, however, in finding such a substance was chiefly due to the circumstance that most of the chlorids of these metals are either too volatile, or begin to suffer decomposition at too low a temperature; it being requisite that the absorbing substance, and the newly formed chlorid of the same, should bear to be heated sufficiently to prevent both condensation of water and absorption of carbonic acid, and at the same time avoid a temperature high enough to occasion any appreciable decomposition of the chlorid.

This question of temperature became, therefore, a prominent one in the investigation, as evidently the success of the process must depend, in a great degree, on the proper management of the temperature of the absorbing substance, within such limits as might be found to give satisfactory results. Hence, my first step was to devise means to secure the necessary control of the temperature of that part of the combustion tube which should contain this substance.

1.



For this purpose was constructed a sheet-iron air-bath or chamber, A, fig. 1, provided with two holes—one on each side—to

[3] *Loc. cit.*

receive the combustion tube, and a tubulure in the top for a thermometer. One end of the air-bath is made to rest on the combustion furnace, and the other, which projects a few inches from the front of the furnace to make room for a lamp, is supported by a leg resting upon the table. The bulb of the thermometer is placed in a central position, in the interior of the bath, close by the side of the combustion tube.

The temperature of the air-bath, and consequently of the substance contained in the combustion tube within, is easily regulated by means of a Bunsen's burner placed under the front end of the bath, as shown in fig. 1. With the exception of the air-bath, the apparatus employed is the same as that used in the analysis of substances containing sulphur, a full description of which is given in the papers above referred to.

The substance that I have found best adapted to absorb the chlorine, for substances easily combustible, is brown oxyd of copper, prepared by precipitation with potassa and ignition over a gas flame.

Difficultly combustible substances, like chloroform, are not completely burnt in oxygen in contact with asbestos alone, but require the presence of a body having affinity for chlorine; otherwise there is formed a liquid body, difficultly volatile,—probably a chlorid of carbon,—which condenses in the vacant part of the tube, from *b* to *c*, fig. 2, and which cannot be entirely burnt off and save the analysis. In such cases the absorbing substance is mixed with the asbestos occupying the back part of the tube, where the combustion takes place. It is evident that oxyd of copper would not answer for this purpose, as at so high a temperature dichlorid of copper would be formed, which, being insoluble in dilute acids, would interfere with the determination of the chlorine. Oxyd of zinc has been found to give good results with such substances.

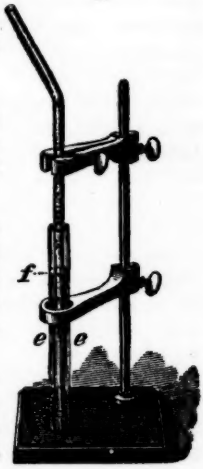
The preparation of the combustion tube, and the arrangement of the mixture of asbestos and the absorbing substance, is the same—except in the case last mentioned—as in the analysis of substances containing sulphur, as shown in fig. 2, viz., the space between *a* and *b*, about 10 inches in length, is packed with pure asbestos; between *b* and *c*,—a space of about two inches,—being left vacant, a plug of asbestos is placed at *c*; the space between *c* and *d*, 4 to 5 inches in length, is filled with an intimate mixture of asbestos and brown oxyd of copper; and, finally, a plug of asbestos is placed at *d*.

2.

After the combustion, the chlorid, together with the excess of oxyd, is extracted from the asbestos by means of dilute nitric acid.

To facilitate the removal of what may adhere to the sides of the tube, the apparatus shown in fig. 3 will be found serviceable as in the analysis of sulphur compounds.

3.



I. *Experiments with Oxyd of Lead and with Oxyd of Copper, placed in the anterior end of the combustion tube, as absorbents of Chlorine in the analysis of substances difficultly combustible.*

The substance selected for analysis, as a test of the process for that class of bodies which are difficultly combustible, containing but a small percentage of hydrogen, was commercial chloroform. The preparation employed was first subjected to redistillation.

Its boiling point was found to agree essentially with that assigned to pure chloroform in Gerhardt's *Traité de Chimie.* When the usual tests were applied, no impurity could be detected.

Experiment 1.—A mixture of oxyd of lead and asbestos was placed in the anterior end of the combustion tube, between *c* and *d*, fig. 2, as previously described. As chlorid of lead was supposed to bear a pretty high temperature, without volatilization or decomposition, the use of the air-bath was omitted in this experiment, and the oxyd gently heated with a small flame from the combustion furnace. The combustion had not proceeded far when it became apparent, from deposition of minute drops of liquid on the sides of the vacant part of the tube,—from *b* to *c*, fig. 2,—that the combustion of the chlololform was incomplete, although no doubt could exist as to the presence of an excess of oxygen. This deposit of liquid, which, as already stated, was supposed to be a chlorid of carbon, was found to be difficultly volatile, suffering partial decomposition, and leaving on the tube a brown deposit, which was not entirely removed by ignition in a stream of oxygen. The high temperature employed to burn off this deposit occasioned excessive heating of the posterior end of the mixture of lead oxyd and asbestos; and this may have been the cause, to some extent, of the excess in the determinations of carbon and hydrogen, although subsequent analyses indicate that the sample of chloroform under examination contained a larger percentage of these elements—particularly of the latter—than belongs to pure chloroform. This experiment gave 11·47 per cent of carbon, and 1·87 per cent of hydrogen. Theory

gives 10·07 per cent of carbon, and 0·85 per cent of hydrogen. The mixture of asbestos and oxyd and chlorid of lead was removed from the tube, and treated in the usual manner with a solution of bicarbonate of soda to obtain a soluble chlorid. This operation was found extremely tedious. Even after treatment for more than two weeks, with occasional fresh portions of the bicarbonate and frequent agitation, the decomposition of the lead chlorid was still found to be incomplete, and the operation was abandoned. As this is given in the text books as a good process for the separation of chlorine from chlorid of lead,[1] I am led to presume that in this case the excess of heat employed gave rise to the formation of an oxychlorid, which is, doubtless, more slowly acted upon by the bicarbonate. This single experiment does not, therefore, prove that oxyd of lead may not be employed in this process with good results, when used for easily combustible substances, and excessive heat is avoided. But it will, unquestionably, be found preferable to use a substance which will give *directly* a soluble chlorid.

Experiment 2.—This experiment was conducted as the last, with only this difference, viz. that oxyd of copper was substituted for the oxyd of lead. No better results, however, were obtained. The reappearance of the difficultly volatile liquid in the vacant part of the tube, while there was assurance of there being no deficiency in the supply of oxygen, served to confirm the impression gained by the preceding experiment,—that chloroform could not be completely burnt in oxygen alone, but that a substance having affinity for chlorine would have to be mixed with the asbestos, at the point where the combustion takes place.

II. *Experiments with Oxyd of Zinc, mixed with the asbestos in the posterior part of the combustion tube, as absorbent of Chlorine in the analysis of substances difficultly combustible.*

As already indicated, the chief object of this set of experiments was to determine whether the presence, at the point where combustion takes place, of an oxyd capable of combining with the chlorine would have the effect to prevent the formation of the difficultly volatile liquid above mentioned, and thus remedy that defect in the process.

Experiment 1.—In this experiment, three grams of oxyd of zinc were intimately mixed in a mortar with the quantity of asbestos necessary to fill the space between *a* and *b*, fig. 2, and that part of the tube then packed with this mixture in the usual manner. A similar mixture composed of asbestos and only one gram of oxyd of zinc was placed between *c* and *d*. The space between *b* and *c* was left still vacant, in order to be able to observe the effect. On account of the volatility of the chlo-

[1] H. Rose, Chimie Analytique, new French edition, p. 801.

rid of zinc, it was deemed advisable to retain the use of the air-bath to control the temperature of the anterior portion of the combustion tube, which, in this experiment, was not allowed to exceed 160° C. The result was, as anticipated, that no such condensation of liquid between *b* and *c* occurred. In order to gain from this experiment some idea of the degree of volatility of chlorid of zinc under such circumstances, the two columns of asbestos were treated for chlorine, separately. The solution obtained from the anterior column was found to contain but a trace of chlorine, giving only a milkiness with nitrate of silver; showing that the chlorid of zinc does not travel far through a column of asbestos from the point where the flame plays directly on the tube.

Results of the Analysis.—0·2067 gram of chloroform gave 0·0798 of carbonic acid, 0·0276 of water, and 0·7372 of chlorid of silver.

			Calculated.	Found.
Carbon	C_2	12	10·0671	10·5273
Hydrogen	H	1	0·8473	1·4514
Chlorine	Cl_3	106·2	89·0856	88·0455
			100	100·0242

Experiment 2.—In this experiment, the whole length of the combustion tube from *a* to *d* was packed with a mixture of asbestos and four grams of oxyd of zinc. The temperature of the anterior end of the combustion tube was regulated, as in the previous experiment, by means of the air-bath.

Results of the Analysis.—0·1339 gram of chloroform gave 0·0506 of carbonic acid, 0·0156 of water, and 0·4768 of chlorid of silver.

			Calculated.	Found.
Carbon	C_2	12	10·0671	10·3062
Hydrogen	H	1	0·8473	1·2733
Chlorine	Cl_3	106·2	89·0865	87·9014
			100	99·4809

These two analyses, agreeing as they do so closely, indicate that the chloroform analyzed contained larger percentages of carbon and hydrogen,—especially of the latter,—and a correspondingly smaller percentage of chlorine than the theoretical quantities; occasioned, probably, by the presence of some impurity. This view is supported by calculations made on the assumption that the excess might have arisen from volatilization of chlorid of zinc, or from incomplete absorption of the chlorine; which would make the chloroform contain from two to six per cent more than the theoretical quantity of chlorine. These results are regarded, therefore, as satisfactorily establishing the utility of this process in the analysis of chloroform. But the

analysis of this body, containing as it does eighty-nine per cent of chlorine, and only eighty-five hundredths of one per cent of hydrogen, must be considered as an extreme case, and does not prove the process a good one for other classes of substances.

The next step, therefore, was to determine whether the process would be equally efficient in the analysis of substances rich in hydrogen, the combustion of which would give rise to the formation of a large quantity of hydrochloric acid. The substance selected for analysis, to settle this question, was chlorid of amyl.

III. *Experiments with Oxyd of Zinc, as an absorbent of Chlorine in the analysis of substances rich in Hydrogen.*

In these experiments the oxyd of zinc was employed in the same manner as above described for the analysis of chloroform. The chlorid of amyl, which was the subject of analysis, was prepared in the usual manner. Its boiling-point was 102°, 8 corrected.

The following results of two analyses with oxyd of zinc indicate that this oxyd combined with and retained some of the carbonic acid. This result was not anticipated, as in the analysis of chloroform the determination of carbon was uniformly slightly in excess.[6]

The results of these two analyses are as follows:—

1.—0·1922 gram of chlorid of amyl gave 0·3513 of carbonic acid, 0·1854 of water, and 0·2528 of chlorid of silver.

		Calculated.		Found.
Carbon	C_{10}	60	56·3910	49·85
Hydrogen	H_{11}	11	10·3383	10·72
Chlorine	Cl	35·4	33·2707	32·47
			100·	93·04

[6] Since the above was written, I have observed upon reviewing my notes,—not only of experiments with oxyd of zinc, but also with oxyd of copper, that in every analysis in which I made note of carbonization, or blackening of the asbestos in the combustion tube,—which may sometimes occur from too rapid distillation of the substance, or, what amounts to the same thing, a deficiency in the supply of oxygen,—there was a loss in the determination of the carbon, and generally, also, in that of the chlorine; while the hydrogen would agree pretty nearly with the theoretical quantity. I am, therefore, at the present writing, inclined to suspect that the carbonization may have had some connection with the deficiency in the carbon determinations in these instances, although the blackening would readily and completely disappear as soon as a sufficiency of oxygen was supplied. The momentary blackening of the asbestos occurred in both of the analyses of chlorid of amyl with oxyd of zinc, but, as already intimated, was not regarded at the time of serious consequence, as similar phenomena in the analysis of hydrocarbons by my process were generally attended with good results. It may, therefore, remain an open question, whether the oxyd of zinc may not serve a good purpose in the analysis of substances of the class now under consideration.

2.—0·1657 gram of chlorid of amyl gave 0·3314 of carbonic acid and 0·1608 of water.

		Calculated.		Found.
Carbon	C_{10}	60	56·3910	54·56
Hydrogen	H_{11}	11	10·3383	10·74
Chlorine	Cl	35·4	33·2707	

IV. *Experiments with Oxyd of Copper, as absorbent of Chlorine in the analysis of substances rich in Hydrogen.*

In these experiments, for the reason previously stated, the oxyd of copper could only be placed in the anterior end of the combustion tube, where it might be maintained at a tolerably low temperature. After two or three experiments,—which were but partially successful,—it became apparent that the range of temperature within which oxyd of copper could be made serviceable to absorb the chlorine was probably rather limited.

It was observed, for example, that at 150° to 160° even brown oxyd of copper, which had been but gently ignited, would fail to absorb nearly all the chlorine, and consequently the determination of the carbon, and sometimes that of the hydrogen, would be in excess. In one experiment, in which the oxyd of copper was kept at about 153° C., its appearance had suffered no change, and it was found to contain only 8·29 per cent of chlorine, or only about one quarter of the theoretical quantity. When a sufficiently high temperature is employed, on the contrary, the posterior end of the column of oxyd of copper and asbestos has the appearance of being entirely changed into yellow chlorid of copper, the rest of the column remaining, for the most part, of its original dark color.

In another experiment, with the oxyd of copper kept at a temperature of about 160°, only about fourteen per cent of chlorine was obtained.

In both of these experiments the carbon determination was considerably in excess, and in one of them the hydrogen also. The oxyd of copper employed had been strongly ignited.

Before proceeding further with these somewhat random experiments, it was deemed advisable to *determine* the temperature at which chlorid of copper begins to give off chlorine, in order to know how far it would be safe to raise the temperature of the air-bath in conducting an analysis. By making use of the air-bath to regulate the temperature of the chlorid of copper, this determination was easily made. During the heating of the chlorid, a current of air from the air-gasometer was admitted through the tube in which it was contained.

Observations.—At 243° not a perceptible trace of chlorine was given off. After the lapse of fifteen minutes, at 250°, the nitrate of silver into which the gas was conducted, was observed to be

slightly milky; this may, therefore, be taken as about the temperature at which chlorid of copper begins to suffer decomposition. At 267°, a solution of nitrate of silver was instantly precipitated.

Thinking that perhaps the small quantity of chlorine evolved under these circumstances might be taken up again and retained if oxyd of copper were present, and possibly, also, that in that case a higher temperature might be safely employed,—to make the conditions of the experiment conform in this particular to those which exist in an analysis, all but one inch of the chlorid of copper was removed from the tube, and in its place was put a mixture of asbestos and oxyd of copper, occupying a space of four inches in length, forward of the chlorid. The experiment was then repeated. Prolonged heating in a current of air, and afterwards in oxygen, during which the thermometer rose to 350°, produced no reaction with nitrate of silver. From this it appears that the chlorine, which was given off below this temperature from chlorid of copper, when this is mixed with oxyd of copper, is absorbed and retained by the latter; hence, that so high a temperature as 350° may be safely employed for the air-bath in conducting an analysis by this process.

Analysis 1.—In this analysis the oxyd of copper employed was prepared in the ordinary way and strongly ignited. The space in the tube occupied by the mixture of asbestos and oxyd of copper was five inches in length, and contained just five grams of the oxyd. During the experiment, the temperature of the air-bath was maintained at about 350°. At the close of the combustion there was no appearance of chlorid of copper, except in the first half-inch at the back end of the column of the mixture of oxyd of copper and asbestos; showing that the temperature employed was favorable for rapid and complete absorption of the chlorine.

Results of the Analysis.—0·1682 gram of chlorid of amyl gave 0·3486 of carbonic acid, 0·1633 of water, and 0·2233 of chlorid of silver.

			Calculated.	Found.
Carbon	C_{10}	60	56·3910	56·522
Hydrogen	H_{11}	11	10·3383	10·761
Chlorine	Cl	35·4	33·2707	32·773
			100	109·056

Analysis 2.—The oxyd of copper employed was of the same preparation as that used in Analysis 1. The space occupied by the mixture of asbestos and oxyd of copper was only $3\frac{1}{2}$ inches in length, but contained the same quantity, viz. 5 grams of the oxyd of copper, as used in the previous analysis. The temperature of the air-bath ranged from 250° to 253°. At the close

of the combustion, it was found that all but $\frac{3}{4}$ inch at the forward end of the column of mixed asbestos and oxyd of copper had the appearance of containing chlorid of copper. By comparison with the corresponding observation in Analysis 1, it will be seen that the appearance of the chlorid extends over more than five times the space in this analysis that it did in the former, showing that with strongly ignited oxyd of copper a temperature higher than 250°, even as high as 350°, is more favorable for the absorption of the chlorine. The following results of the analysis, however, are equally accurate with those of the preceding analysis.

0·1669 gram of chlorid of amyl gave 0·3457 of carbonic acid, 0·1612 of water, 0·2213 of chlorid of silver.

		Calculated.		Found.
Carbon	C_{10}	60	56·3910	56·489
Hydrogen	H_{11}	11	10·3383	10·785
Chlorine	Cl	35·4	33·2707	32·732
			100	100·006

Analysis 3.—Under the impression that an oxyd of copper which had been less strongly ignited might be effectual to absorb the chlorine at a lower temperature, I employed in this and the two following analyses a preparation of brown oxyd of copper, obtained by precipitation with potash and ignition over an ordinary gas flame. In this analysis the temperature of the air-bath ranged from 150° to 158°. The space occupied by the asbestos mixture was four inches in length, and contained three grams of the oxyd. Although the results of the analysis indicate that the temperature of the air-bath was too low, they also show, by comparison with the results obtained in operating with strongly ignited oxyd at about the same temperature of the air-bath (see p. 163), that the brown oxyd is decidedly preferable in respect to the temperature required. This was also shown by the appearance of the oxyd after combustion,—the newly formed chlorid being confined, in the case of the brown oxyd, to a much shorter space.

Results of the Analysis.—0·1640 gram of chlorid of amyl gave 0·3504 of carbonic acid, 0·1562 of water, and 0·1884 of chlorid of silver.

		Calculated.		Found.
Carbon	C_{10}	60	56·3910	58·268
Hydrogen	H_{11}	11	10·3383	10·582
Chlorine	Cl	35·4	33·2707	28·360
			100.	97·210

Analysis 4.—Used the same preparation of oxyd of copper as in analysis 3, viz., the brown oxyd. Temperature of the air-

bath reached 170°. Slight carbonization occurred just at the close of the combustion, from extending the heat backward too soon, under a wrong impression that the substance was all burnt. Were it not for this circumstance, it is believed that this would have been a good analysis, although the temperature of the air-bath was kept so low. That a higher temperature of the bath is desirable, however, is shown by the fact that the chlorid of copper appeared diffused over a space of 2½ inches. The length of the column of mixed asbestos and oxyd of copper was only four inches in this experiment, containing *but one gram* of the oxyd.

Results of the Analysis.—0·1568 gram of chlorid of amyl gave 0·3195 of carbonic acid, and 0·1522 of water.

		Calculated.		Found.
Carbon	C_{10}	60	56·3910	55·574
Hydrogen	H_{11}	11	10·3383	10·784
Chlorine	Cl	35·4	33·2707	

Analysis 5.—The oxyd of copper employed was of the same preparation as that of analyses 3 and 4. The temperature of the air-bath, however, was considerably higher, ranging from 240° to 247°. The mixture of asbestos and oxyd of copper occupied a space of five inches in length, but contained only two grams of the oxyd. At the close of the combustion there was no appearance of chlorid of copper, except at the back end of the column, a space ¾ of an inch in length.

Results of the Analysis.—0·1631 gram of chlorid of amyl gave 0·3383 of carbonic acid, 0·1557 of water, and 0·2157 of chlorid of silver.

		Calculated.		Found.
Carbon	C_{10}	60	56·3910	56·542
Hydrogen	H_{11}	11	10·3383	10·607
Chlorine	Cl	35·4	33·2707	32·649
			100·	99·798

It can hardly have escaped observation, that the quantity of oxyd of copper or oxyd of zinc required to absorb the chlorine by this process is extremely small, in consequence of its being uniformly diffused through a large mass of asbestos; hence it is obvious that but little of a solvent is needed to extract the chlorid. In this respect the new process bears a striking contrast to the old one, which involves the use of a large quantity of lime, necessitating a corresponding quantity of acid, and introducing disagreeable manipulation, which tend to increase the liability to error.

I have not yet tried the process recently described by Carius,[*]

[*] Annalen der Chimie und Pharmacie.

as the difficulty which I had found in obtaining tubes that would bear the pressure incident to his process for the determination of sulphur gave no encouragement of better success in the use of his process for the determination of chlorine, which is performed in a similar manner, although more complicated.

The advantage which my process affords, of being able to determine the three elements, carbon, hydrogen, and chlorine at a single combustion, without the introduction of any difficult or hazardous manipulation, induces the belief that it will be found preferable to any other that has been devised.

ART. XXV.—*The Vowel Elements in Speech;* by SAMUEL PORTER, of Hartford, Conn.

THE division of the alphabetic elements into vowels and consonants is one which grammarians have ever been compelled to recognize, however hard they may have found it to mark the distinction by satisfactory definitions. The nature of the vowels is such, somehow, that every word must contain at least one of them. The same is true, for the most part, of syllables as well as words. The consonants *l*, *n*, *r*, *m*, do indeed occasionally take the place of a vowel in a dependent, unaccented syllable, leaving the written and originally spoken vowel silent,—as ab*l*e, trif*l*e, id*l*e, harden, mutton, reckon, heaven, Britain, often, permit, forlorn, curtail, themselves, give'em, and the like,—sometimes properly, and sometimes by a slightly incorrect pronunciation. But no word, and no syllable under a full accent, is without a vowel. The rare exceptions which may occur, as in some of the Slavic tongues and the *r* of the Sanskrit, cannot be allowed to invalidate the general rule. The difference between vowel and consonant is a fundamental one: the obvious difference in function rests upon a difference in essential nature,—what that is will be developed, in the sequel, as incidental to our main purpose.

Grammarians, at this day, are generally agreed as to the importance of what they call the physiological analysis of the alphabetic elements. The positions, motions, &c., of the vocal organs, as producing the sounds, need to be ascertained in order to a successful investigation of those relations of the phonetic elements which appertain to the science of language. The *Lautlehre* is understood to involve consideration of the *physiologische Process.* The term *physiologico-mechanical* would express the full idea more accurately than *physiological.* The phrase "mechanism of speech" may well denote the objective matter of inquiry. It is upon mechanical relations among the vocal

elements that the laws of syllabication and euphony and the processes of phonetic transformation depend far more than on auditory impressions. Without a true physiological analysis, investigation into the laws of phonetic change must be merely empirical: only so far as an approximation thereto is realized, can comparative philology fairly claim rank as a science. In the matter of a "standard alphabet," and for the phonetical description of strange languages, as well as, finally, for orthoëpical and elocutionary purposes, the importance of such analysis will hardly be questioned. To one who should derive his ideas on the subject from *Le Bourgeois Gentilhomme* of Molière, such studies might indeed seem idle and ridiculous; but those who know anything of the subject see in it a matter of practical as well as scientific interest, sufficient to invite and to warrant the thorough and minute treatment which alone can yield valuable results.

As respects the vowels, such investigations have hitherto had but partial success. While there is a general agreement on many leading points, there is still on many others a wide diversity of views. Observations wanting in precision have led to a corresponding vagueness in the use of terms. Mere incidental concomitants have been mistaken for essential matters. Few systems have even professed such completeness as to find a place in an orderly scale for every vowel sound; none pretending to be thus complete has presented claims so demonstrably valid as to compel a general acceptance. Dr. Brücke, of Vienna, the author of a most thorough and able treatise on phonology, remarks that "the formation of the vowels still presents to us considerable theoretical difficulties, which it will take a long time perhaps to solve in a satisfactory manner."[1] Prof. Max Müller, in the second series of his Lectures, treats the physiology of the vowels with a good deal of particularity, but makes no attempt to present a complete and exhaustive scheme. In short, a true system of the vowels has thus far remained a desideratum.

The difficulties which beset the subject are considerable. In the first place, a special kind of practice with the elements, singly and separately, must have become familiar to the investigator. Not less essential is the careful training of the ear to the just discrimination of articulate sounds. Then, the experiments—to be made and observed again and again—will need much careful attention, and call for some ingenuity of contrivance. A slight variation of the "physiologic process," so slight as to be hardly perceptible, or not at all perceptible without observation of a special kind, will often result in a marked dif-

[1] Cited at second-hand from Prof. R. L. Tafel's *Investigations into the Laws of English Orthography and Pronunciation* (New York, 1862).

ference in the character of the sound produced. Attention will be needed to distinguish with invariable certainty compound elements from simple, mixed from pure; and especial care in order to eliminate whatever is only incidental, or even purely accidental, and so to seize upon what is really essential.

When all is done so far, there remains the task of making one's self understood by others. The clearest and most thorough exposition will be but labor lost upon those who have undiscriminating ears and loose habits or incorrect modes of pronunciation; and the number of such among otherwise well-educated men—linguists and grammarians with the rest—is by no means small. Local and national diversities of pronunciation are another barrier to a mutual understanding in these matters. The Teuton who speaks *cab* and *cub* nearly like *cap* and *cup*, *bag* like *back*, *food* like *foot*, with the *b*, *d* and *g*, as merely softened forms of *p*, *t*, *k*, after his own vernacular, even if at all aware that the sound of these letters is not precisely the same in German as in English, will certainly stumble at our physiological analysis; and as it is with consonants, so with vowels; only the difficulty is far greater.[1] We have in this country not a few provincial, that is, local, or as we Americans say, sectional, peculiarities of pronunciation; and in Great Britain such exist in a more marked degree, in the case of the higher as well as the lower classes. Such diversities of usage, quite unsuspected it may be, are liable to render the examples employed for illustration ineffectual to any other result than a thorough misunderstanding.

I am led to offer my views on the subject because I believe that I have so far overcome the obstacles first named as to have hit upon the key to a true system of the vowels, and feel in duty bound to encounter the difficulties involved in the task of exposition.

I would not put forward my scheme in an attitude of antagonism toward the other systems or half-systems which have gained acceptance. I would have it regarded as completing what was fragmentary, and explaining what was but half understood,—by bringing to view certain new relations,—and as having its own substantial correctness confirmed by the ground it furnishes upon which to reconcile the conflicting diversities of other schemes. If, on minor points, my positions shall in

[1] Baron Kempelen says, (Mechanismus der Sprache, 1791,) "there are there [in Germany] whole provinces where the people have never in their lives spoken a *b*, and cannot do so even for once." This is especially true of Saxony. A weakening, when not an entire falling off, of the muffled "sonant" quality of the medials, *b*, *d*, *g*, in whatever part of the word, is a very general characteristic of the Germans. Hence, their phonologists generally, and Max Müller with them, disallow this quality as distinctive, though Kempelen (a Viennese) strongly insisted on it as such, and illustrated it clearly by the experiment of a flageolet blown within a bladder.

any case appear open to question, or even be fairly convicted of inaccuracy, I shall be well content, provided I succeed in demonstrating the correctness of the system in its leading features. I have not, however, been careless of the details.

The following principles are the key to the system:—

1. All the vowels are articulated primarily between the tongue and the palate. Some of them, those usually called labials (*old*, *ooze*, *all*, &c.), are further modified by the action of the lips. All are thus either palato-linguals simply, or else labio-palato-linguals; and the latter consist of a palato-lingual part, capable of being employed by itself, and of a labial part which is dependent on and super-added to the other.

2. The articulation[1] is effected (1) as between the tongue and the palate in the following manner:—The organs are so disposed, and the muscles of the tongue, with those also of the soft-palate, so put into action, as to make a firm tube, or passage, fitted for the reverberation of the sound which comes from the larynx; and this passage so differs for all the vowels, as to modify the sound in a peculiar manner for each—the cases excepted, of course, in which the same palato-lingual articulation makes two distinct vowels as used with and without the labial modification. (2) The labial modification is effected by a firm contraction and more or less protrusion of the lips together with a rigid tension of the cheeks, so as to cause a further reverberation of the sound, and thus give the vowel a different character to the ear: the sound is reverberated through two passages or cavities instead of one.

3. The vowels—labial and non-labial—are assorted into *groups*, according as the palato-lingual passage extends more or less forward. The passage is either just at the throat, or is extended and lengthened by joining the lateral margins of the tongue to the sides of the palate, till finally the tube so formed reaches quite forward under the dome of the hard-palate and nearly to the tip of the tongue; though it is to be remarked that, for the anterior groups, the place is more precisely determined upon the palate than upon the tongue, owing to the extensile property of the latter. By "the throat" I mean the orifice bounded by the posterior extremity of the palate above and the root of the tongue below.

4. Each group thus determined embraces individual vowels

[1] The words *articulate*, *articulation*, as applied to speech, point primarily to the *articuli*, small members or joints,—that is, the syllabic utterances,—of which speech is composed; but have been technically applied to the elements into which syllables themselves may be resolved; and by French Grammarians are usually restricted, without good reason, to the consonant elements; this is seldom done by English or German writers. It is further allowable to use the terms, as I do here, with particular reference to those mechanical adjustments of the organs which give to the several elements their distinctive character.

differing in *degree* as more or less open or close. These differences are effected, in the palato-lingual passage, by approximating more or less to the palate the part of the tongue at the place of the articulation, especially at the front terminus of the passage; the passage may at the same time be narrowed more or less, as more or less of the margin of the tongue is put into contact with the borders of the palate. The labials will need no other or further criterion; for, in their case, the more or less openness of the lips will correspond to that between tongue and palate.

The scheme does not contemplate a precise admeasurement of open and close as between vowels of different groups; it requires no such comparison; it deviates from other systems in this, especially, that it assigns to different places, and thus ranges under separate groups, vowels which have been commonly viewed as differing merely in degree of openness.

The degree of openness as between tongue and palate is not only to be distinguished from the greater or less opening of the jaws, but it is to be noted that the two do not always coincide, and, especially, that the close labials (*awe*, *owe*, *ooze*, &c.,) involve a wider separation of the jaws than the corresponding open and non-labial vowels (*nor*, *not*, *fully*, &c.). A decided labial modification requires, absolutely, a considerable opening of the jaws, that the stretched cheeks may wall the passage.

A glance at the diagram and the table a few pages forward, will give the reader a more definite general idea of the scheme. *Some preliminary points* require attention before proceeding with the details.

1. As to the *number of vowels capable of being produced*, there is no certain limit in nature. The variations in degree of openness are obviously infinite; the variations as the terminus of the palato-lingual passage is more or less in advance are in like manner infinite; and each variation causes a greater or less difference in the quality or character of the sound. Thus, to say nothing of the labial modifications, and excluding diphthongs of course, we have, theoretically, an infinite multiplied by an infinite, for the number of possible simple vowel sounds. We have nothing which answers to the laws of melody and harmony in music whereby to determine the intervals of the scale. All we can do is to mark certain points, as if by lines of latitude and longitude, and make no account of intermediate gradations any further than to refer them to the nearest of these points. As for the number to be recognized in a system, much will depend on the special purpose in view. My object obviously requires a scheme both comprehensive and minute,—and will exact minuteness of orthoëpical detail in the way of illustration.

Languages vary greatly in the number of vowels they em-

ploy. In all our modern tongues we have many more than the three original vowels of the Sanskrit and the Gothic,—though we can hardly doubt that these three admitted severally considerable latitude of variation. In English, we recognize as distinct many more than we have separate characters for; while over and above these, we may notice slight variations, as due to the influence of associated consonants, or in connection with varying accentuation or emphasis; and, in the pronunciation of different persons, and even of the same person at different times, we observe appreciable shades of difference in what will be usually regarded as the same vowel.

2. In all the vowels alike, *the sound proceeds from the larynx*, being struck out upon the chords of the glottis, which, when drawn near each other to the proper interval and duly contracted, are set into vibration by air forced through from the lungs; the sound is then modified into this or that vowel by reverberation through a passage of this or that description. In vowels whispered, the vocal chords are so adjusted that we have aspiration, or breath-sound, instead of tone; but the sound originates in the larynx, and not, as in the consonant *f*, for example, at the place of articulation.

Sound produced in the larynx, intonated or aspirated, does not always take the form of a vowel. So it does not in the sound (*hm*) made in clearing the throat; in which case it undergoes a peculiar modification, but makes no vowel. Laryngeal sound is heard also in many of the consonants,—*l*, *m*, *r*, *v*, *z*, for example,—without being at all modified vowel-wise.

3. The larynx opens directly into *the pharynx*, which is a musculo-membranous sac through which the breath from the larynx must pass on its way to either the mouth or the nares, behind both of which the pharynx is situated, being properly a continuation of the esophagus, and separated by the *velum palati* from the mouth. The form and disposition of this organ will of course be affected by the movements and positions of the tongue and soft-palate; but we know not that the organ has any special agency in giving their distinctive character to the several vowel elements; at all events, its part must be quite a subordinate one. Inasmuch as the *velum palati* and the root of the tongue form the front wall of the pharynx, and as I hold that the vowel-tube begins always as far back as the throat, and that it is, moreover, essentially modified by varying adjustments of the *velum*,[4] so far my theory involves, indeed, a modification of the pharynx itself, and one that differs more or less for different vowels. That there is an action of the fore-part of the

[4] See the positions of the soft-palate marked on the diagram. Upon this matter, my independent examinations led me to conclusions not differing from those established by the elaborate and ingenious experiments of Dr. Czermak, of Vienna.

pharynx in all the vowels, was observed by Prof. Leon Vaïsse, of Paris; and it was regarded by him as giving them that general character or quality in which they differ from the consonants.[8] My view is that the whole vowel-tube is concerned in producing this general character, by the very same action which gives distinctive character to each several vowel.

4. After the vocal current has passed the palato-lingual passage, it has still to traverse a further portion of the oral cavity; and in every case the sound will be in some degree modified or colored according to the disposition of those anterior parts; but, except in the proper labials, the effect will not amount to a change in the essential character of the vowel. The mouth may be more or less open, that is, the jaws and lips more or less far asunder, and yet the vowel be the same. The cheeks may even be tensely drawn close to the teeth, very much as in the more open of the labials, and still leave so predominant the essential character given by the palato-lingual passage, that we recognize the modification only as a different resonance given to the same vowel. In the word *hail*, for example, we should by such means give to the vowel a more full and sonorous quality in the recitation of "H*ai*l, holy light," than would be suitable in the expression, "H*ai*l-fellow, well-met." We can even utter and recognize the vowel *a*h, f*a*r, with the lips in position for f*oo*t or *oo*ze. The tongue, beyond and just forward of the proper terminus of the palato-lingual tube, may happen to flare or bend away but slightly, and so the observer be led to mistake the place of articulation as further forward than it is in fact, unless he correct himself by trial with different positions of the organ, which he should be careful to do.

5. Some who read this may need, at the outset, to be disabused of the habit,—arising from the customary use of the terms "long" and "short,"—of *confounding quantity*, or time, *with quality*, or character as respects articulation. Let it be observed then, that no amount of mere prolongation will change, for instance, the so-called short vowel in *full, pull, foot*, into the corresponding long in *fool, pool, rule, food;* nor will any process of curtailment convert the latter into the former. This is but an example of what is true universally of the vowels in the English, and to a greater or less degree in all languages. The tendency to variation of quality when a vowel is lengthened or shortened, is natural and universal.

Philologists have been accustomed to define the difference of long and short, in the vowels, as one of duration merely; but have found themselves forced—in a real inconsistency with this—to treat the distinction as one of fundamental importance in

[8] See the article, PAROLE (Physiologie et Grammaire), from the pen of Prof. Vaïsse, in the Supplement to the *Encyclopédie Moderne* of the MM. Didot.

etymology: the differing relations of the long and of the short, which they must and do recognize, are quite unaccountable on their view of the case. If we lay open the physiological ground of a difference here in quality, we do so much to place etymological science upon the right basis.

6. There are questions concerning *the relation of the vowels to tone or pitch.* Have the vowels each what may be called in any sense their natural pitch? This, if so, can help little to a knowledge of their proper vowel character, which remains the same under every variety of pitch. Is the peculiar character of each vowel to be explained as a certain combination of harmonic notes? This, if so, will not help much, in our physiological inquiries, till we have a better understanding of the mechanical conditions upon which such combinations depend in other cases. Prof. Max Müller, in reporting the discoveries of Helmholtz on this point, tells us the vowel quality is to be explained as exactly analogous to the *timbre* by which instruments, as the violin, flute, harp, &c., are distinguished one from another. (Lect. on the Sci. of Lang., 2nd ser., pp. 127–8: Am. ed.) But we have voices differing in *timbre*,—the reedy voice, the flute-like voice, &c., differing as do the instruments to which we liken them,—yet each voice uttering all the vowels, and giving to every one its peculiar character. We cannot, therefore, accept this as an adequate explanation. As I have not seen the work of Helmholtz, and as the account of his investigations which Müller gives is probably very imperfect, I shall express no opinion upon their merits or bearing. They run on a different line of inquiry from that which I have here in hand, and neither supersede it nor interfere with it.

There are two or three facts under this head, which, if not already familiar, can be easily verified. *First:* If we utter in succession any two or more of the simple vowel elements, we shall find them naturally taking a different pitch one from the other; and, referring to our series of groups arranged according to the place of articulation as reaching more or less forward upon tongue and palate, we find that, in passing from any vowel in the scale to one further forward and similar in degree of openness, the voice rises in pitch, while, proceeding in the other direction, it as naturally falls. Of this fact I have an application to make, by and by, in reference to what Dr. James Rush calls the *vanish* of the vowels. The following I propose as a physiological explanation of the fact. The movements of the tongue have an influence upon those of the larynx, through the intermediation of the hyoid bone, a moveable fulcrum with which both organs are connected by muscle and ligament. The connection is such that a movement of the tongue will require a reädjustment of the muscles of the larynx, to keep the latter

organ in the same place and condition as before, and so to preserve the same pitch. Hence, though the two successive vowels *can* be uttered on the same pitch, a special intention will be required for it, and it is much more easy and natural to vary the pitch. *Second:* Every singer knows that only with certain vowels can the extremely high notes be fairly reached, and only with certain others again the lowest. The connection just mentioned between tongue and larynx will suffice to explain this also, though we would not affirm that there is no other cause concurrent therewith. *Third:* the vowels cannot each be uttered on every pitch with equal purity of tone, and there would seem to be one certain key for each, on which the purest tone is heard,—the purest musically we mean, that is, the most free from discordant intermixture. The same cause above mentioned may have an agency in this case also; but I think that here the effect may be due primarily to the form and dimensions of the vowel-tube.

7. I would have distinctly understood *what I do not, as well as what I do, affirm.* I do not say that the character of the several elements depends on the length of the vowel-tube, or on its shape, or the structure of its parts; I believe it, however, to depend on a combination of all these. I do not fail to perceive that, even in the anterior groups, the position and action of the tongue are, in each case, attended with a particular configuration and action of the soft-palate, which is important as affecting the quality of the vowel. I will not deny that the reverberation against the dome of the palate, at a point forward of the proper palato-lingual tube, may bear an important part in impressing upon some of the elements their proper character.[*] I state simply that the vowel-passage is made as I have described, and that the forward terminus of the palato-lingual part is in fact as I have indicated in each case.

I also recognize peculiar styles of enunciation as affecting somewhat the place of the articulation. There is the guttural style, retracting the throat and thus moving back the place of all the vowels. There is also the thin, dental style, just the opposite of the other. But, in each of these modes, the several vowels will be found to maintain their relative places, in substantial accordance with the scheme. I have aimed to assign their places, as definitely as possible, as they are in what I consider the most natural and least affected style of utterance, and as the places at which the peculiar quality of each element is most distinctly brought out and sharply discriminated.

[*] Prof. Henry N. Day, who has given much attention to the subject, seems disposed to regard the peculiarities of the several vowels as proceeding wholly from this cause. See his article on *English Phonology* in the *Biblical Repository* for Oct. 1843.

In the *physiological analysis*, which I am now to give, *of the simple vowel elements*, I shall arrange them in *nine groups*,—according as the palato-lingual tube reaches farther or less far forward,—which I shall designate as the *a*, *å*, *o*, *u*, *ö*, *ä*, *e*, *é*, *i* vowels, or groups of vowels. Under each group I note *four degrees*, more or less close or open, which I call *close*, *middle*, *open*, *open-depressed*, and indicate by numerals affixed as superiors; thus: a^1 (close), a^2 (middle), a^3 (open), a^4 (open-depressed). Labials will be distinguished by an ^{l} affixed to the figure for the degree, as: o^{1l}, u^{1l}, $å^{2l}$, &c.

The diagram and the table, here inserted, depend of course, for their explanation, upon the analysis in detail which will follow. In the *diagram*, we have the median line of the palate from root of front teeth to root of uvula,—the hard-palate marked by the heavier stroke, and the soft-palate by dotted lines which indicate the varying configuration for different vowels, particularly *a*, *o* and *u*. The letters mark the termini of the palato-lingual tube as on the upper or palatal side; short dashes mark the termini below as on the median line of the tongue, and thus, of course, show the several degrees of openness,—they indicate, also, the direction of the vocal current at the termini.

Diagram of Palato-lingual Positions.

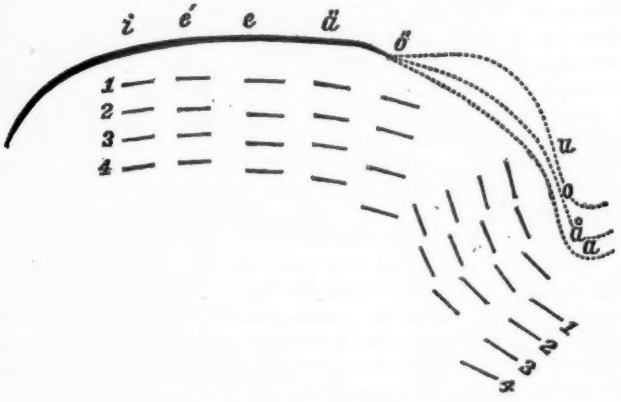


Table of the Simple Vowel Elements.

GROUP I.

a^1:—last, ask, chant; Fr. la, lira, *a*.
a^2:—father, calm; Fr. dame, malade, caver.
a^3:—ah, arm, charge; Fr. âme, bas.
a^4:—Broad pron. of psalm, balm, pass, &c.; do. of Fr. âme, bas.

GROUP II.

$\mathring{a}^{1l}$:—war, lord, *awe*, pause.
$\mathring{a}^{2l}$:—all, water, long, daughter; first element in boy, voice.
$\mathring{a}^{2}$:—salt, although, cross, horror.
$\mathring{a}^{3}$:—sod, nor, off, what, knowledge.
$\mathring{a}^{4}$:—Low Ger. *a*; wrong pron. of war, lord, glory, forth, scorn, and of first elem. in joy, rejoice.

GROUP III.

o^{1l}:—main element in note, toe, low, loaf, door, mourn, beau, hautboy; Fr. ôter, eau, pivot; Ger. Ofen, lobt, Mond.
o^{1}:—Wrong pron. of note, toe, &c.
o^{2l}:—opinion, agony, propose, mellow; Fr. obéir, noble, porter, mot, dominer; Ger., kochen, Holbe, Morgen.
o^{2}:—Wrong pron. of coat, toad, stone, &c.
o^{3}:—not, dot, folly, knock, proper; Ger. Gott, flott, Ross.
o^{4}:—Wrong pron. of door, oar, board, &c.; Fr. encore, corps, alors, aurore.

GROUP IV.

u^{1l}:—fool, pool, moon, move, shoe, soup; main and final elem. of union, few, view, beauty; Fr. rouler, vous; Ger. Schule, Stuhl, gut.
u^{2l}:—full, pull, bosom, woman, should, good, foot, book; final elem. in our, now, round; vanish of woe, row, roll; Fr. coup, bout, bourse; Ger. lustig, Schuld, Bund.
u^{2}:—Wrong pron. of pull, foot, book, &c.; Ger. durch, Butte.
u^{3}:—fulfill, willful; wrong in foot, soon, put, &c.
u^{4}:—Qu: Scotch gude, sune, bluid, puir, (for *good*, *soon*, *blood*, *poor*,)? Initial of dew, new, tube, lute, suit, rude, &c.—u^{4} or u^{3}.

GROUP V.

$\ddot{o}^{1l}$:—Ger. schön, König, Vögel; Fr. jeûne, heureuse, feux.
$\ddot{o}^{2l}$:—Ger. Wörter, möchte; Fr. leur, jeune, amateur.
$\ddot{o}^{2}$:—mercy, virtue, girl, myrtle, earl, pearl, earth.
$\ddot{o}^{3}$:—up, but, cousin, rough, dove, done, flood; Fr. de, le, ce, and (nasal) un, brun.
$\ddot{o}^{4l}$:—Fr. beurre, coeur.
$\ddot{o}^{4}$:—Broad pron. of church, work, &c; first elem. of *our*, bound, now, also of ice, my, right.

GROUP VI.

$\ddot{a}^{1}$:—Ger. Mädchen, täglich, wäre, gäbe, leben, geben, gelegen; Fr. après, scène, plaie, jamais, faire, père; Eng. their, fair, parent.
$\ddot{a}^{2}$:—care, there, prayer, hair, pair,—$\ddot{a}^{2}$ or $\ddot{a}^{1}$; Ger. rächen, dämmern.
$\ddot{a}^{3}$:—at, cat, man, sad, hap.
$\ddot{a}^{4}$:—Drawling of cat, man, &c.; Fr. (nasal) vin, fin, cousin.

GROUP VII.

e^{1}:—Main elem. in fate, name, great, vein, grey, hail, pay, gaol, guage; Ger. mehr, jeder, ledig, See; Fr. perhaps in some cases the "open *e*."
e^{2}:—nitrate, climate, parliament, and usually initial in fate, name, &c.; Ger. fertig, Keller, Liebe, Vater; Qu: Fr. aimer, maison, &c.?
e^{3}:—get, egg, red, mend.
e^{4}:—Qu: Fr. tête, bête, fête?

GROUP VIII.

é^1:—Fr. bont*é*, cit*é*, j'*ai*, aimer*ai*.
é^2:—guine*a*, valle*y*, carri*ed*, cit*y*, and (vulgar) Americ*a*; Fr. c*e*tte, t*e*lle, ciel, *ai*mer, m*ai*son.
é^3:—Ger. d*e*nn, B*e*tt; goodn*e*ss, coll*e*ge, &c.
é^4:—Swedish long *é*, as in Carlén.

GROUP IX.

i^1:—mach*i*ne, f*ie*ld, *ea*t, *e*ve, d*ee*p; Fr. av*i*s, l*i*re, am*i*e; Ger. M*i*ne, m*i*r, w*i*der.
i^{1l}:—Fr. r*u*se, Gr*ue*; Ger. *ü*ber, Schüler.
i^2:—d*i*vine, veh*i*cle, mit*i*gate, mandar*i*n; the vanish of n*a*me, h*ai*l, &c., also of *i*ce, m*y*, &c., and of *oi*l, b*oy*, &c.; Fr. am*i*, fidèle, f*i*er, v*i*f; Ger. m*i*t, b*i*tten, n*i*cht.
i^{2l}:—Fr. *u*ne, r*u*de; Ger. Glück, w*ü*nschen. Initial of *u*nion, *view*.
i^3:—p*i*n, h*i*t, s*i*n, w*i*ll.
i^4:—Drawling of p*i*n, w*i*ll, &c.; initial in a Yankee pron. of d*o*, r*u*de, sm*oo*th, &c.

Physiological Analysis of the Vowel Elements.

I. THE *a* VOWELS.—For these, the place of articulation is between the root of the tongue and the extremity of the soft-palate, that is, at the throat. That no part of the tongue but the root is essentially concerned in the articulation, may be easily ascertained: for the tongue can be variously rolled and twisted without materially marring the vowel sound. The tongue may lie loose upon the floor of the mouth, except that the whole will naturally so participate in the movement of the root portion as to be somewhat raised in the close vowel. There are no labials in this group.

Degree 1.—*Vowel a*^{1}. This, the quite close vowel of the group, is proper in such words as staff, graft, pass, ask, last, chant. (Princ. of Pron.,[7] §§ 5, 6.) It is the closest *a* in French, as in l*a*, l*i*r*a*. It has a strong tendency to pass into *ä*^{2} (cat),—the natural position of the tongue and also that of the soft-palate being nearly the same for both,—but the two sounds are to be clearly discriminated.

Degree 2.—*Vowel a*^{2}. The proper Italian *a*, and the ordinary *a* in French, as *a*blir, m*a*lade. In English, the Italian *a*, as we call it, in f*a*ther, *a*rm, &c., is variously heard, but this form is to be regarded as the more elegant in most cases.

Degree 3.—*Vowel a*^{3}. The open or broad *a* in French, as â*m*e, b*a*s, gr*â*ce, p*a*ssant,—prolonged when under the circumflex.

Degree 4.—*Vowel a*^{4}. The open French *a* may by some be pronounced in this form, that is, with the utmost depression of the root of the tongue. So, also, the broad Low German *a*, and the Scotch broad *a*, in man, &c., though more commonly as *å*^{4}.

[7] ***Principles of Pronunciation***, prefixed to the new edition (1864) of Webster's Dictionary.

In English, the *a* may sometimes be heard, improperly, thus broad and open, in words like ps*a*lm, b*a*lm, p*a*ss.

II. The *å* Vowels.—The posterior part of the tongue is somewhat raised and is adjusted on each side to the lower portion of the soft-palate to form the vowel-tube, which thus extends upward and forward a little way from the throat, and directs the vowel current obliquely upward. In this condition of the back-tongue, the tip and fore part will be naturally retracted, and more or less so as the vowel is more or less close.

A retraction of the tongue is natural also when the jaws are set widely apart for a very open *a* vowel; and this may be a special ground of the easy and frequent transition between vowels of that group and this, which actually occurs in the history of vowel changes.

Degree 1.—*Vowel* $å^{1}$. W*a*r, w*a*rm, *awe*, l*o*rd, f*o*rm, *o*rder, p*au*se. (Princ. of Pron., §§ 7, 25.) The labial modification is decided, the lips being strongly contracted and protruded, and the cheeks drawn inward. Indeed, pure vowel-utterance in the very closest form is almost impossible without the lip-contraction. If we attempt it, we make an approach to the soft German *g* in *Tage*.

Degree 2.—*Vowel* $å^{2l}$. The difference between this and the preceding is not very strongly marked to the ear; but in some words the associated consonants make the vowel less close, as *a*ll, w*a*ter, w*a*nder, s*o*ng, sw*a*llow.

This vowel is the first element of the diphthong b*oy*, v*oi*ce.

Vowel $å^{2}$. Like the preceding except in the absence of the labial-modification: as s*a*lt, *a*lthough, h*o*rror, s*o*ft, s*o*lve, cr*o*ss, g*o*ne, c*au*ght. (See Prin. of Pron., § 21.) Let a practised elocutionist try such an example as, "All the horrors of war," which contains this and the two preceding vowels, and, his voice and ear being without fault, he will not fail to recognize each as different from the others.

Degree 3.—*Vowel* $å^{3}$. S*o*d, pl*o*d, n*o*r, *o*ff, wh*a*t, kn*ow*ledge; differing but slightly from the preceding.

Degree 4.—*Vowel* $å^{4}$. Here properly falls the broad Low German *a*. Here, also, we find the initial element in a certain flat pronunciation of j*oy*, rej*oi*ce, &c., heard not unfrequently,—$å^{4}$ in place of $å^{2l}$. Some speakers use this in place of the properly close vowel in w*a*r, *a*ll, l*o*rd, *awe*, &c., and even for the long *o* in gl*o*ry, gl*o*rious, and other words.[8]

The Scotch m*o*n, bl*aw*, sn*aw*, (for m*a*n, bl*ow*, sn*ow*,) obviously

[8] Gardiner, in his *Music of Nature*, (p. 61, Am. ed.,) says of Macready, "By aiming too much at distinctness, he incurs a false pronunciation of the vowels, which proceeds from his drawing back too much the corners of his mouth; so that we have *scarn* for scorn, *go farth* for go forth, *harrible! harrible!* for horrible! horrible!" *Drawing back the corners of the mouth* is identical with entire absence of labial modification; the vowel actually heard was, I doubt not, $å^{4}$.

belong to the *å* group, but, to which degree, my opportunities have not been such as to enable me to determine.

It is to be remarked that the open-depressed degree suits in all cases with long quantity, and thus, in every group except the first (*a*), is liable to appear as a substitute for the close degree, which is naturally long in all but the first group, while the simply open degree (No. 3) is, with the same exception, the one least of all fitted for long quantity.

III. The *o* Vowels.—The palato-lingual passage is extended one step further, to a higher point along the *velum palati;* and, in direction, is inclined still more highly upward; the *velum palati* itself is higher and more arched. As the back-tongue is thus raised, the fore-part will naturally rise also. Like as in the preceding group, the tongue will naturally be retracted more or less according to the degree of closeness.

The close and open or middle *o* correspond to the long and short *o* in most of the languages of Europe; thus we have *o* close and long in our n*o*te, open and short in n*o*t. There is, however, in the Italian and some other tongues, especially the Danish and Swedish, a distinction of so-called open and close not identical with this; and to confound that with this would be a serious mistake. The Italian close *o* (*o* chiuso, stretto) is described by Dietz, A. J. Ellis, and others, as nearer to the *u* (r*u*de, f*u*ll). The Italian open *o*, (aperto, largo,) probably lies a little on the other side of our *o* and nearer to the *å*. The distinction, which holds alike in the long and the short vowels—croce, bocca (close), modo, dotto (open),—is at this day a nicety of pronunciation not generally recognized or regarded except in the purest style of the language as spoken by native Italians.

Degree 1.—*Vowel o*^{1l}. N*o*te, *o*ld, *o*ver, &c., that is, the "long o"; French, trône, ôter, repos, clos, dépôt, pivot, *au*, *eau*, &c.; German, *O*fen, l*o*bt, M*o*nd, &c.

The English "long *o*" is almost always diphthongal, or compound, taking a vanish in another vowel, which is commonly of the *u* group (f*u*ll, f*oo*d); as plainly appears in h*oe*, b*ow*, n*o*, b*owl*, *owe*, l*ow*, &c. Followed by *r*, as in b*oa*rd, st*o*re, g*o*re, *oa*r, r*oa*r, the vanish is a labial vowel of the *ö* group (b*u*t, *e*rr, Fr. *eu*). In spoke, broke, *o*ver, als*o*, &c., quickly uttered, the vanish, if any, is imperceptibly slight. In English as spoken by foreigners, the long *o* without the vanish, in accordance with their own vernacular, is usually a noticeable peculiarity. In the customary English utterance, the main element is so disguised by the vanish, that its real character is not ordinarily observed, while the labial modification still further obscures its relation to the open vowel (deg. 3) of the group.

Vowel o^{1}. Sometimes heard as an improper mode of pronouncing the long *o* in English,—with the vanish, but without

labial modification: one of the affectations of some public speakers. It might possibly be identified as an Irish or Scottish peculiarity, or both.

Degree 2.—*Vowel* o^{2l}. Heard in syllables which take a sort of secondary accent, as *o*pinion, *co*temporary, ag*o*ny, mulatt*o*, mel*ow*, propose, proceed. And in a considerable number of words, such as st*o*ne, c*oa*t, t*oa*d, l*oa*f, &c., the best taste will, perhaps, prefer this to the extreme close *o*, not neglecting, however, something of the vanish; also in t*o*rn, l*o*rn, b*oa*rd, d*oo*r, &c.

We have here the shorter *o* in French, as *o*béir, noble, &c. The German short *o* may, as I think, fall sometimes here and sometimes in the third or open degree.

Vowel o^2. An extremely improper pronunciation of a class of words just alluded to, c*oa*t, st*o*ne, t*oa*d, thr*oa*t, wh*o*le, l*oa*f, &c., quite common in America, and more especially in the rustic dialect of New England. The fault is commonly described as consisting simply in the omission of the vanish (Princ. of Pron., § 20), but the non-labial character and the more open degree are in fact equally essential. B*oa*rd, d*oo*r, *oa*r, t*o*rn, &c., are also frequently and faultily so pronounced.

Degree 3.—*Vowel* o^3. N*o*t, d*o*t, h*o*p, &c., which with those under $\mathring{a}^3$ (n*o*r, *o*ff, s*o*d, &c.,) are the "short *o*" in English. The distinction between o^3 and $\mathring{a}^3$, though slight and usually not regarded by orthoëpists, is actually existent in practice, but depends mostly, we believe, on the influence of consonants associated.

Here, I think, belongs the shortest German *o*, Gott, Ross, flott; as also the French, sotte, culotte, folle, &c.

Degree 4.—*Vowel* o^4. Differs not greatly, but I think appreciably, from the $\mathring{a}^4$. Here belongs, if I mistake not, the French encore, corps, alors, aurore. We hear it in one of the several mispronunciations of b*oa*rd, *oa*r, t*o*rn, f*o*rth, &c.

IV. The *u* Vowels.—The palato-lingual passage reaches another step forward on the tongue, and to a higher point upon the soft-palate; the vocal current is nearly vertical in direction, and the soft-palate is arched upward extremely: the group stands as the terminus of an ascending series from the throat.

In this group, the tongue, in passing from open to close, is perceived to be distinctly elevated as well as retracted.

Degree 1.—*Vowel* u^{1l}. The closer and usually longer *oo*, as f*oo*d, &c.; the *o* in d*o*, &c.; *oe* in sh*oe*, &c.; *ou* in y*ou*, &c.; the main and final part of the compound in *u*nion, v*iew*, f*ew*, b*eau*ty, &c. Whether the *u* in r*u*de, r*u*in, &c., should take a slight initial sound of another vowel, is made a matter of question. I think it strikes, or should do so, a more open vowel of the group, and then falls upon this; as it does also in d*ew*, n*ew*, t*u*be, l*ure*, s*uit*, &c.

This is the long *u* vowel of most European languages. In French, it is the long *ou*; the letter *u*, as alone, having early gone over from this to a vowel group further forward.

The labial contraction is closer in this vowel than in the close *o*. The vowel has a peculiar mellow smoothness, and imparts the same character to the *o*, when added thereto as a vanish. It is possible to utter the palato-lingual part of this vowel without the labial modification, but not smoothly and with perfectly pure vowel quality.

Degree 2.—*Vowel* u^{2l}. F*u*ll, p*u*sh, bosom, sh*ou*ld, g*oo*d, f*oo*t, &c. In the proper pronunciation of these words, the lips are contracted to nearly or quite the same degree as for the close *o* simple. It is the final element in the diphthong *our*, n*ow*, r*ou*nd, and the usual vanish of w*oe*, l*ow*, r*o*ll, &c., the long *o*. In French, we have it in c*ou*p, b*ou*t, b*ou*rse, &c., that is, the shorter *ou*. It is the short or middle *u* of the German and of most of the languages of Europe.

Vowel u^{2}. This non-labial is frequently used in America, improperly, in place of each of the two preceding, as in f*oo*t, s*oo*t, r*oo*t, r*oo*f, s*oo*n, b*oo*k, sh*oo*t, f*u*ll, p*u*t. If I mistake not, it is the proper form of the shortest *u* in German, as in d*u*rch, &c.

Degree 3.—*Vowel* u^{3}. The unaccented f*u*lfill, willf*u*l, &c.; also, t*o*, d*o*, &c., when unemphatic and somewhat slurred; also formerly common and still to be heard in New England in some, if not all, of the class of words just specified, f*oo*t, s*oo*n, &c. Young misses who mince their words will pronounce t*wo*, as well as t*oo*, in this way. But slight change in the action of the organs is needed to convert this utterance into either a short *i* or a French *u*.

Degree 4.—*Vowel* u^{4}. Heard, as I incline to think, in the Scotch g*u*de or g*ui*d, s*u*ne, s*u*ld, bl*u*de or bl*ui*d, d*u*re, &c. (for *good*, *soon*, *should*, *blood*, *door*, &c.), with perhaps a vanish in another (an *i*) vowel. Occurs in no case in well-spoken English, except as the initial element in the best pronunciation of d*ew*, n*ew*, t*u*be, l*u*te, &c. (Princ. of Pron., § 30.)

V. The *ö* Vowels.—The palato-lingual tube reaches yet further forward, but hardly beyond the extreme fore-part of the soft palate. The edges of the tongue join the borders of the palate about as far as to the hinder teeth, and the tip of the tongue is naturally further forward than in the more close of the degrees in the preceding group.

In this and in all the remaining groups, the degrees of close and open are made by the greater and less elevation or depression of the tongue, and not at all by its retraction.

Degree 1.—*Vowel* o^{1l}. A sound strange to English ears; the long *ö* in German, as schön, König; the long close *eu* in French, as j*eû*ne, heur*eu*se, f*eu*x.

Degree 2.—*Vowel* $\ddot{o}^{2l}$. The shorter German *ö*, as Wörter, möchte; the French *eu* in l*eu*r, j*eu*ne, p*eu*r, &c.

Vowel $\ddot{o}^{2}$. There is, in English, a class of cases in which *e*, *i*, *y*, or *ea*, followed by *r*, takes properly this articulation; as *er*r, m*er*cy, v*ir*tue, b*ir*d, g*ir*l, m*yr*rh, m*yr*tle, *ear*l, p*ear*l, *ear*th, *ear*n. It is very common, indeed, to give here the sound of *u* in *u*p, b*u*t, or b*u*rn, *u*rge. But orthoëpists agree, for the most part, that a different utterance in these cases is sanctioned by the best usage,—without being well agreed, however, as to its precise character. As I here describe it, the vowel differs from the French l*eu*r simply in the absence of the labial modification. (See Prin. of Pron., §§ 14, 18.)

Degree 3.—*Vowel* $\ddot{o}^{3}$. The *u* in *u*p, b*u*t, &c.; *o* and *oo* in d*o*ne, c*o*mpany, fl*oo*d, &c.; *oe* in d*oe*s;—unaccented syllables tend to this sound, as alt*a*r, offer, tap*i*r, zeph*y*r, verb*a*l, bedl*a*m, ball*a*d, meth*o*d, &c. Frequent in English, but rare in other European tongues. In French, occurs nasalized in *u*n, br*u*n, &c., and is the so-called "*e* feminine," as in d*e*, c*e*, d*e*mand*e*, dam*e*, when not elided,—unless this takes somewhat of a labial modification.[9]

This English vowel is described by some German grammarians as approaching the short German *ö* and lying between that and the short *o*.[10] By some English phonologists, it has been called "the natural vowel," and by others "the neutral vowel"; and for the most part they seem at a loss how to locate it in their systems. Dr. Rapp, in his *Physiologie der Sprache*, styles it the *Urlaut*, *Urvocal*, the original, or primitive vowel. It is by some described as "the unmodified vowel." To perceive that this vowel is not mere vocal tone unmodified, we need but to notice the fixed, rigid position of the tongue in the utterance. The vocal element, or tone, in the consonants *v*, *z*, &c., is unmodified vowel-wise, but is clearly unlike the vowel in question. Only, when the vowel is slurred and almost elided, as in perform, token, or Fr. serez, delà, there is probably no determinate vowel modification.

Degree 4.—*Vowel* $\ddot{o}^{4l}$. The best French orthoëpists have distinguished the sound of *eu* before *r*, in b*eu*rre, c*oeu*r, &c., as a broad and open one; as do also some of the best instructors. It is recognized by the ear as an approach to an open-depressed *a* or *å*, and falls exactly into the place here assigned it.

Vowel $\ddot{o}^{4}$. This, as by itself, we have only to note as a broad, flat, drawling utterance of the *u* in *u*p, b*u*t, ($\ddot{o}^{3}$), and by some clerical speakers affected in ch*u*rch, w*o*rk, &c.

[9] Prof. L. Vaïsse ranks this *e* as a "labio-palatal." Palsgrave (1530) ascribes to it a nasal quality: says the reader should "sodeynly depresse his voyce when he cometh to the soundynge of hym, and also sounde hym very moche in the noose." *L'Eclaircissement, &c.*: ed. Genin.

[10] See Mätzner, Eng. Grammatik, i, 14; and Fiedler, p. 115.

This element is, however, important as the initial of two diphthongs, viz., *ou* or *ow* (*our*, *now*), and long *i* (*ice*, *kite*), according to the best usage. The Scotch give the long *i* as $ö^2+i^1$; some of the North of England dialects as $å+i$; we sometimes hear it as a^2+i^2, sometimes as $ä^2+i^2$, not to speak of other variations.

VI. THE *ä* VOWELS.—The vowel-tube, reaching a step further, fairly laps upon the hard palate above; and the borders of the tongue will there meet either the teeth or the gums or palate on each side. The tip of the tongue will be naturally more advanced than for the preceding group.

Degree 1.—*Vowel* $ä^1$. The longer German *ä*, as Mädchen, wäre, &c. The long and grave German *e*, as leben, geben, gelegen, is the same, unless somewhat less close.[11] Also, the "open-grave" *è*, so-called, in French, as après, scène, jamais, faire, père, &c. In other languages of Europe, also, *e* sometimes takes a similar sound, improperly described as "open." Thus in Spanish, as we are taught in Sales' Grammar, "before *n*, *r*, *s*, *z*, in the same syllable, *e* is pronounced more open, as in the English words *care*, *snare*." In Italian, *e* has such a so-called open sound in numerous words.

In English, this vowel occurs only as followed by *r*. *Heir*, *their*, *fair*, *fairy*, *parent*, *pair*, *bear*, &c., take properly either this, or else the middle degree ($ä^2$), usage being diverse; and many give improperly the "long *a*" sound (e^1). I incline to a discrimination, and would give to *heir*, *their*, *parent*, and some others, the close degree, and the middle to *there*, *where*, *hair*, *prayer*, *care*, *pear*, and others. The influence of some professed orthoëpists, whose obtuseness has led them to ignore the distinction between this vowel and the "long *a*," has tended to expel the sound from the language. Still, as respects the English "long *a*" itself, the usage is less settled and uniform than is generally supposed. In some quarters it even takes the vowel sound here in question, which once regularly belonged to it,[12] and this cause may have helped to obscure the distinction

[11] The distinction between the *e* in leben, &c., and the proper long *e* (see the *e* group), is noted by Jacob Grimm and others, but would seem not to be universally observed at the present day. Heyse, in his *Schulgrammatik*, describes this *e* as similar to the *ä*. Possibly it would come nearer to our e^4, which is somewhat liable to be confounded with $ä^1$.

[12] Originally, the *a* in English had a proper *a* sound, and it was only by gradual change that the "long *a*" of the present time came about. Two hundred years ago, Dr. John Wallis and Bishop Wilkins knew no other English sound of *a* than what they both describe expressly as the Italian sound, long and short. The *ai*, which now almost invariably takes the "long *a*" sound,—*pail*, *main*, not differing from *pale*, *mane*,—was then usually a proper diphthong, the same now used in the word *aye*, and sometimes heard yet in Isa*ia*h, Sin*ai*, *aisle*. In the gradual change from the original to the present sound, the vowel-group we now treat of would naturally be taken on the way; and, in the case of the diphthong, probably the initial element first came into this group, and then, dropping the final and taking a

just alluded to. Such a use is marked by Alex. M. Bell, of Edinburgh, as "an oratorical and especially a pulpit Scotticism," as "in *nation*, *education*, *gracious*, &c." (Elocutionary Manual, &c., Edinb., 1859.) But, as I am told that it is a marked characteristic in the pronunciation of some English public speakers, the Rev. James Martineau among others, and as I have heard it not unfrequently from native Americans, I am disposed to regard it rather as a relic of olden time which traditional habit has preserved to some extent, and more especially in the pulpit and on the stage.

It is not unusual to describe the vowel in question as identical with the *e* in *met* prolonged. The truth is that to prolong the *e* in *met* without change of quality is difficult, and the attempt is apt to produce this vowel as a matter of fact; yet the two are really different in vowel quality. The description is good as a practical rule, but not to be accepted as a true analysis.

Degree 2.—*Vowel* $\ddot{a}^2$. In treating of the vowel above, we have remarked sufficiently upon this, as concerns the English. In German, the shorter *ä* takes this sound in many words, as r*ä*chen, d*ä*mmern, though in perhaps the greater number it does not differ essentially from the short *e*; but there is a want of uniformity in its pronunciation by the Germans themselves.

Degree 3.—*Vowel* $\ddot{a}^3$. The so-called short *a* in English, as *a*t, c*a*t, m*a*n. Not heard in German. In French, modern usage inclines somewhat to substitute this for the more proper close *a*.

Degree 4.—*Vowel* $\ddot{a}^4$. This is heard in the drawling utterance of the short *a* (c*a*t) in the genuine Yankee dialect, which also substitutes it for $\ddot{a}^1$ or $\ddot{a}^2$, as in wh*e*re, h*a*ir, &c., and, besides using it conspicuously in *a*re, makes it the initial element in *our*, c*ow*, n*ow*, r*ou*nd, the whole compound being nearly $\ddot{a}^4+\ddot{o}^3+u^3$. The vowel involves such an action of the *velum palati* as superadds a decided nasal quality. Fully nasalized, it is the French *in*, as v*in*, f*in*, cous*in*, &c.

close form, passed over to the present "long *a*," which lies in the next group forward. Examples of the transitional usage were observed,—within the present century, of course,—by the eminent phonologist and linguist, Dr. Erasmus Rask, to such extent as to give him the idea that this was the usual pronunciation of the English long *a* and *ai*. In his *Danish Grammar for the use of Englishmen* (ed. Repp), he notes two sounds of Danish *e*, as like the "French *é fermé* and *è ouvert*," and says: "The *é* ferme, or close *e*"—which we have yet to describe—"is very frequent in Danish, but not of frequent occurrence in English; still it is found in such words as *their*, *vein*, which have a different sound from *there*, *vain*,"—these, of course, being meant as like the "open *e*." He had also just said: "The open *e* is exactly like the *æ*, but usually short," and had described the *æ* as equivalent, long and short, to the English *a* in *sale* and *ai* in *said*, respectively.

I do not forget that Dr. Rask is regarded by some as more nice than wise in these matters; but somewhat unjustly, I suspect. In the second edition of his Danish Grammar, edited by Repp, and professedly a faithful reproduction of the first, we find no such distinction made between *pale* and *pail* as Mr. Marsh ascribes to him. (See *Lectures on the English Language*, by G. P. Marsh, first series, p. 285.) Did Mr. M.'s usual accuracy fail him in this case?

VII. The *e* Vowels.—The passage is extended still further on the hard-palate and the tongue, and the tip of the tongue will naturally be found further forward.

Degree 1.—*Vowel* e^1. The "long *a*" in English, as in *a*le, f*a*te, gr*ea*t, v*ei*n, h*ai*l, d*ay*, &c.; also, the more usual German long *e*, as in m*e*hr, j*e*der; the other, in l*e*ben, &c., has been already described as $\ddot{a}^1$. Possibly, some one of the two or three or more varieties of the "open *e*" in French may not differ materially from this.

This vowel, in English, more commonly takes a vanish in an *i* vowel (p*i*que, p*i*n, &c.); as it plainly does always in s*ay*, r*ay*, h*ai*l, &c.

Degree 2.—*Vowel* e^2. Occurs in English in lightly accented or unaccented syllables; as nitr*a*te, carbon*a*te, clim*a*te, parl*ia*ment, lapid*a*ry, comparative, &c. In such a case as *e*dge, the consonant inclines the vowel to this instead of the open sound of *e* in g*e*t, though *e*dge would never be made close like *a*ge. The long *a* is usually struck upon this degree, falling quickly upon the closer sound for the main part, and ending off with the vanish in *i*; e. g., n*a*me, c*a*me, p*ai*n; but in quick utterance, the closer sound is not given at all.

This is the usual shorter *e* of the German, accented and unaccented, as f*e*rtig, K*e*ller, Lieb*e*, V*a*ter.

Degree 3.—*Vowel* e^3. The English short *e*, so-called, as in g*e*t, *e*gg, r*e*d. We do not hear precisely this in the shortest French *e*, c*e*tte, trompette, nor in the German, as d*e*nn, B*e*tt, &c.;—*denn* is not just the English *den*, nor is *sechs* the same with *sex*.

Degree 4.—*Vowel* e^4. To this I am disposed to assign the French *ê*, as in t*ê*te, f*ê*te. In English, the long *a* may be sometimes drawled into this form, and singers do this sometimes with the short *e*, as in s*e*lf, t*e*n, for example; but in each case it is a flat and faulty pronunciation.

VIII. The *é* Vowels.—The passage reaches well on to the forward part of the hard-palate and the tongue.

Degree 1.—*Vowel* $é^1$. The close *é* and *ai* of the French, as bont*é*, cit*é*, j'*ai*, aimer*ai*. The sound may sometimes be given to the English long *a*, but such is not the usual pronunciation.

Degree 2.—*Vowel* $é^2$. The French "open acute" *e* and *ai*, as c*e*tte, c*ie*l, *ai*mer, m*ai*son; also heard in certain unaccented syllables in English, as guin*ea*, vall*ey*, carr*ied*, coll*e*ge, r*e*sist, pr*e*pare, app*e*tite, l*e*vel, bus*y*, cit*y*, and in the vulgar *a* final, as Americ*a*, Cub*a*, Eliz*a*. The vowel is somewhat difficult to discriminate from e^3, in the preceding group.

Degree 3.—*Vowel* $é^3$. To this belongs, I think, the German d*e*nn, B*e*tt, &c., and the shortest French *e*, as trompette, &c. It occurs, unaccented, in g*oo*dness, knowledge, trumpet, &c.,—the right sound here being not the short *i*, nor the regular short *e*, but intermediate.

Degree 4.—*Vowel é*[4]. The Swedish long *é*, as Carlén, from the best information I have, would appear to be correctly described as this vowel. Dr. Thomas's description of it (Webster's Dictionary, new ed., p. 1634) as "a sound resembling that of short *i* prolonged," would bring it very near to this.

IX. THE *i* VOWELS.—The most advanced group in the scale of palato-lingual position. The borders of the tongue are applied to the palate, not indeed clear to the tip as for the consonant *s*, but as far forward as can be, seeing the place of articulation is a vowel-tube reaching from the throat, instead of simply a point as it is for the consonant. It is also to be observed that the middle and back part of the tongue is raised to a position somewhat like that for the *u* vowels, and with a similar, or even greater, arching up of the soft-palate.

I give this as the precise arrangement which brings out the sound most distinctly and most naturally. But, in all the anterior groups, as before remarked, owing to the extensile structure of the tongue, the articulation may have a determinate and nearly invariable place upon the palate, and yet reach to a variable point on the tongue. Thus, in this case, the tongue may be thrust forward, with the tip and fore part depressed behind the lower teeth; the terminus of the vowel-tube falling further back, of course.[13] The variation is the same as may occur in the so-called dental consonants, *t*, *n*, *d*, which are properly made with the tip of the tongue, but can be uttered by using a part of the tongue considerably further back.

The peculiar shape of the palatal arch, as it converges forward and gives to the passage a rounded form, would seem to bear an essential part in producing the vowels of this group. If they can be imperfectly imitated at a place further back on the palate, it is done only by so shaping the tongue as to make a somewhat similar converging and rounded passage.

Degree 1.—*Vowel i*[1]. Mach*i*ne, f*i*eld, *ea*t, &c., and the long *e*, and *ee*, as *e*ve, m*e*ter, d*ee*p, &c.; the long *i* on the continent, as (Fr.) av*i*s, l*i*re, am*i*e, and (Ger.) M*i*ne, m*i*r, w*i*der.

Vowel i[1*l*]. The long *u* of the French, as r*u*se, gr*ue*, and long *ü* of the German, as *ü*ber, Sch*ü*ler. As commonly uttered, it might, if without the labial modification, form a somewhat impure *i*, made such by some admixture of a consonantal *y*. This vowel probably nowhere exists as developed from an original *u* vowel.

Degree 2.—*Vowel i*[2]. The so-called short *i* of the French, as am*i*, f*i*dèle, v*i*f, and of the German, as m*i*t, b*i*tten, n*i*cht. In English, may be heard in v*e*hement, v*e*hicle, d*i*vine, mit*i*gate,

[13] So represented in the diagram in Max Müller's Lectures, second series, p. 134, which is also more seriously at fault in placing the point of approximation quite too far back upon the palate.

&c.: and, in mandarin, capuchin, chlorid, &c., is preferable to the closer sound. It makes the usual vanish of "long *a*" in name, praise, &c., the whole being commonly $e^2+e^1+i^2$; also, the final element of "long *i*," ice ($ö^4+ö^3+i^2$), and of *oi*, *oy*, as toil, boy ($å^{2l}+i^3$).

Vowel i^{2l}. The shorter French *u*, as une, rude, ruban, perhaps a little more open in butte, russe, &c.; and the shorter German *ü*, as Glück, wünschen, Mütter. We have in this—and not in the simple i^2 or i^1—the initial part of the English long *u*, as union, use, tube, mute, and of *you* in *youth*, *you*, &c., and *eau* in beauty. Between this and the main and final element, there is a distinct consonantal *y*. So that the long *u* is $i^{2l}+y+u^{1l}$. I propound this,—with deference of course to the "Autocrat,"—as the secret of how to pronounce the word *view*.

Degree 3.—*Vowel* i^3. The "short *i*" in pin, hit, give, &c. The shortest French *i* may sometimes approach this, as in petite, risque, ville, and the German in bitten, ist; but are hardly, we think, to be ranked here.

Degree 4.—*Vowel* i^4. Heard in an improper prolongation of our short *i* (i^3), sometimes as a faulty general habit, and sometimes used in the way of emphasis; properly, the short vowels admit only the emphasis of force or stress, and the syllable should be prolonged, if it all, only on the consonant. The rustic New England do, rude, smooth, &c., sometimes takes this for the initial element.

Having thus completed the analysis of the simple vowel elements, I will suggest an experiment by which I am willing to abide as *a test of the correctness of my theory*. It is easy to observe, in the first place, that i^3 (it, kin) is more open than i^1 (eat, keen); or e^3 (ell, pen) than e^1 (ale, pane); and, in general, that the so-called short are more open than the corresponding long under the several groups,—which, indeed, has not been unrecognized as a fact by orthoëpists (as see Princ. of Pron., Notes to §§ 2, 8, 11, 16, 25, 31). This point being settled, if then we articulate the series of close vowels in order from front to rear, viz., i^1 (eve), $é^1$ (aimé), e^1 (fate), $ä^1$ (their, Mädchen), $ö^{1l}$ (König, jeûne), u^{1l} (ooze), o^{1l} (oak), $å^{1l}$ (awe), a^1 (past), we shall find the position of the organs such that, if we suppose a wedge inserted between tongue and palate, it would enter further and further with each successive vowel, being stopped at each point by the front terminus of the palato-lingual passage. We have only to use a thin rod, or even a finger, to perceive this. Observe that, at the point of the wedge supposed inserted, there will be constantly found a close position: showing that the vowel station is simply carried back, and that the difference is not one of merely open and close. The same thing may be

done with the series of open vowels i^3, $\acute{e}^3$, &c. (pin, Ger. denn, end, cat, but, willful, not, nor, Fr. bas or Eng. balm),—and the same fact of the regress of the vowel station will be observed. Experiments of this sort, fairly made, seem to me to furnish complete demonstration of the leading principles on which I insist. Further proof will appear in the sequel.

If, on the other hand, we try to arrange all the vowels in a single series on any principle whatever, we find ourselves utterly baffled. If we distinguish them as simply more and less open or close, we find confusion instead of order. Nor should these terms *open* and *close* be applied otherwise than as I have done. It is true they might not unaptly be used to describe the difference as the vowel station moves back toward the throat or forward from it,—even as they might describe the corolla of a flower unfolded down toward the base or only near the tip,—but the terms are wanted to indicate the width of the expansion, and must therefore not be used for the depth. Neither is the confusion escaped by setting the labials in a class by themselves, arranged according to the extent of the labial opening: the labials cannot be all so discriminated, if we include all in actual use, while for the non-labials the difficulty still holds. Nor will any other subdivision answer, which falls short of the groupings, or substantially such, as in the scheme here presented.

[To be continued.]

ART. XXVI.—*On Photo-micrography with the highest powers, as practised in the Army Medical Museum;* by J. J. WOODWARD, M.D., Asst. Surgeon and Brevet Major U. S. Army, in charge of the Record and Pension Division Surgeon General's Office, and of the Medical Section Army Medical Museum.

PHOTOGRAPHY had but just begun to attract attention when the attempt was made by Donné to reproduce microscopic objects by the Daguerrean process; and although the results of these experiments were far from satisfactory, they promised enough to lead to further efforts in this direction, renewed with each step in the gradual improvement of the photographic art. These exertions were crowned by a continual progress, which did not however keep pace with the development of other branches of photography, though it must be admitted that in the hands of the more modern experimenters, and especially of Prof. Gerlach of Erlangen, Jos. Albert of Munich and Dr. R. L. Maddox of Southampton, the success has been such as to guarantee a wide field of usefulness for this method of representation.

In America, the chief experimenters have been Prof. O. N. Rood of Columbia College and Mr. Lewis M. Rutherfurd of New

York. Besides these, mention must be made of the paper of Dr. John Dean of Boston on the Spinal Cord, which is illustrated by photomicrographs reproduced by photolithography. The work of Dr. Dean however was done with magnifying powers not exceeding ten or twelve diameters, while both Professor Rood and Mr. Rutherfurd have experimented with very high powers.

Prof. Rood published a very interesting account of his process in this Journal in 1861.[1] Omitting details, it appears from this paper that in his operations, he used direct sunlight for illumination, and employed ordinary achromatic objectives with or without eye-pieces. The difference between the visual and chemical foci he endeavored to overcome by an alteration of the fine adjustment after the plan suggested by Shadbolt.[2] Prof. Rood thus obtained photographs, chiefly of diatoms, so far as I have been able to learn, with powers as high as the $\frac{1}{7}$th objective, which gave with five feet distance 460 diameters, with about three feet distance and the long eye-piece 1300 diameters. The pictures thus obtained compared favorably with any which have been taken with achromatic objectives. In May, 1865, Mr. Lewis M. Rutherfurd, of New York, published a paper on Astronomical Photography,[3] which contained the following suggestive passages. "The image of a star at the focus of a perfectly corrected objective would be a point, the apex of all conceivable cones having the object glass or parts of it as the bases. This point falling upon a prism would be converted in a line, red at one end and violet at the other, with the intermediate colors in their proper places. If, however, the different colored rays are not all brought to the same focus, the spectrum will no longer be a line, but in the uncorrected colors will be expanded to a brush the width of which will be the diameter of the cone where intercepted by the prism. It will thus be seen that a simple glance at a star spectrum will indicate at once what parts of the spectrum are bounded by parallel lines and consequently converged to one focal point, and what parts do not conform to this condition, and also the amount of divergence. On applying this test I found that an objective of flint and crown in which the visual was united with the photographic focus, (in other words, where the instrument could be focalized on a plate of ground glass by the eye, as in ordinary cameras, and in the heliographs constructed by Dalmayer for the Kew observatory and for the Russian government,) is a mere compromise to convenience in which both visual and actinic qualities are sacrificed."

[1] On the practical application of Photography to the microscope; by Prof. O. N. Rood; vol. xxxii, p. 186.

[2] On the photographic delineation of microscopic objects by artificial illumination. By Geo. Shadbolt, Esq. Quarterly Journ. of Microscopical Science, vol. i, p. 165.

[3] Astronomical Photography; by Lewis M. Rutherfurd, this Journal, xxxix, 304.

"In order to bring the actinic portion of the spectrum between parallel borders, i. e., to one focus, it is necessary that a given crown lens should be combined with a flint which will produce a combined focal length about one-tenth shorter than would be required to satisfy the conditions of achromatism for the eye, and in this condition the objective is entirely worthless for vision." With a telescopic objective constructed on this principle, Mr. Rutherfurd obtained telescopic photographs of such satisfactory quality that he concludes his paper as follows:

"The success of this telescopic objective has encouraged me to hope that an almost equal improvement may be made for photography in the microscope, which instrument is more favorably situated for definition than the telescope, since it is independent of atmospheric conditions. Its achromatic status is easily examined by the spectroscope, using as a star the solar image reflected from a minute globule of mercury. Mr. Wales is now constructing for me a one-tenth objective, which, upon his new plan, is to be provided with a tube so arranged as to admit the removal of the rear combination, and in place of the one ordinarily used, one is to be substituted at will which shall bring to one focus the actinic rays."

This objective was satisfactorily constructed by Mr. Wales (of Fort Lee, N. J.) and Mr. Rutherfurd made with it a number of experiments, full of promise, though his other pursuits prevented him from following out the new plan to its ultimate results.

Such was the condition of photo-micrography in America when it occurred to me to resort to this method of illustration in preparing proper representations of the histological studies of camp diseases which have been made by me or under my direction for the Official Medical History of the War of the Rebellion.

I at once visited Mr. Rutherfurd, whose paper had attracted my attention, and I received from him many important suggestions which I desire to acknowledge in the fullest manner. Among these I may especially mention the plan of constructing the objective above indicated, the use of the ammonio-sulphate of copper, and the suggestion of substituting a properly constructed concave for the eye-piece.

In developing these suggestions the actual manipulations were assigned to Asst. Surgeon and Brevet Capt. Edward Curtis, U.S.A., of the Army Medical Museum, and to his patience, tact and ingenuity I am indebted for the successful issue of the experiments which were undertaken. The results attained have been most satisfactory, excelling, as is confidently believed, anything heretofore done in this direction. This is also the opinion of Dr. Maddox, whose judgment is of the greater value as he is one of the most successful laborers in this direction in Europe.

The principles involved in obtaining successful photographs with the microscope are the following:

1. To use objectives so corrected as to bring the actinic ray to a focus.

2. To illuminate by direct sunlight passed through a solution of ammonio-sulphate of copper, which excludes practically all but the actinic extremity of the spectrum.

3. Where it is desired to increase the power of any objective, to use a properly constructed achromatic concave instead of an eye-piece.

4. To focus on plate glass with a focusing glass, instead of ground glass.

5. With high powers to use a heliostat to preserve steady illumination.

6. Where an object exhibits interference phenomena when illuminated with parallel rays, as is the case with certain diatoms and many of the soft tissues, to produce a proper diffusion of the rays by interposition of one or more plates of ground glass in the illuminating pencil.

Strict adherence to these principles is indispensable to success. In the Museum they have been carried out by the following details:

A camera is not used, a dark room being found most convenient. The operating room has two windows, through one of which just enough yellow light is admitted to permit the movements of the operator. The lower part of the other window is occupied by a shutter about fourteen inches high on which the blackened sash shuts down light-tight. In this shutter is a round hole an inch and a half in diameter, from the inner side of which a brass tube of the same diameter projects into the room. On the outer side of the hole is a rod about twelve inches long, on the extremity of which the microscope mirror is duly centered. Two steel rods attached by hooks to the mirror and passed through the shutter permit its position to be adjusted by a person standing inside of the room, without opening the window. A Silbermann's heliostat standing on a shelf just outside of the window throws the sunlight steadily upon the mirror. Within the room a frame of walnut, ten feet long, is placed on a firm table perpendicular to the window. The microscope stands on the end of this frame next the window, its mirror is removed, being replaced by that outside the shutter. The microscope is placed in a horizontal position, and the tube carrying the diaphragm or the achromatic condenser fits into the tube projecting inward from the shutter by which the sun's light reflected from the mirror outside is admitted. A black velvet hood covers the parts about the stage and objective of the microscope, and thus prevents the leakage of light into the room.

The plate holder is movable backward and forward on the walnut frame on which the microscope stands, its maximum distance from the stage of the microscope being nearly nine feet.

To permit ready focusing at distances greater than the length of the arm, a wooden rod $\frac{3}{4}$ths of an inch in diameter and capable of easy rotation runs the whole length of the right side of the frame. The milled head of the fine adjustment of the microscope is grooved, and a small grooved wheel in the end of the rod permits the two to be connected with a band. The operator standing at any part of the frame can therefore manipulate the fine adjustment by simply turning the wooden rod in his fingers.

The arrangements of light, position of object, coarse adjustment, &c., are made by the operator, who stands by the microscope, which has a suitable eye-piece adjusted, and observes the object in the usual way; afterwards, removing the eye-piece and going to the plate holder, the final focusing is made by means of the wooden rod, the image being viewed with a focusing glass on a piece of plate glass held in the same frame which is to receive the sensitive plate.

The cell containing the ammonio-sulphate of copper hangs outside the shutter over the hole by which light is admitted. It not only excludes the unnecessary illuminating rays, but prevents danger to the objective from the concentrated solar heat and permits the eye of the operator to view the objects about to be copied without fatigue or injury. Latterly a plate of alum has also been used to exclude solar heat especially during any temporary removal of the ammonio-sulphate cell. The chemical processes employed are well known to all photographers. With the above apparatus, it has been found that the best defined pictures are obtained when the distance employed with any objective does not exceed three or four feet.

The achromatic concave used as a substitute for the eye-piece is a combination of somewhat more than half an inch transverse diameter, and about 28° angle, constructed, like the objective, to focus the chemical rays. It increases the magnifying powers of the objective about seven times. It has been found to perform well with both the $\frac{1}{8}$th and $\frac{4}{10}$th.

In photographing the soft tissues or other objects in which illumination with parallel rays produces interference lines, the ground glass is to be placed between the mirror and condenser. Of course, there is considerable diminution of light, but this can be overcome, for the higher powers, by condensing the sun's light on the ground glass by a bulls-eye or other similar contrivance. If the interference lines as seen by the eye do not disappear with one thickness of ground glass, two or more may be used.

The most powerful objective with which photographs have

been taken in the Army Medical Museum is a $\frac{1}{50}$th, manufactured recently for the Museum by Messrs. Powell and Lealand of London. The subject selected for the experiment was *Pleurosigma angulatum*. With the $\frac{1}{50}$th and three feet nine inches distance and without an eye-piece, a picture of a portion of a frustule was obtained magnified 2,344 diameters. This negative readily bore enlargement to 19,050 diameters. The field in the picture is six inches in diameter and is remarkably sharp in the center, but shows considerable curvature and on the edges is quite out of focus. Further experiments with the $\frac{1}{50}$th satisfied us that a greater power could not be advantageously obtained from it.

About the same time experiments were made with the Wales' $\frac{1}{8}$th, due amplification being given by the achromatic concave. It was intended to obtain with this the same power as with the $\frac{1}{50}$th, but, although the distance was reduced to 3 feet, the subsequent measurements showed 2,540 diameters, or about 200 diameters more than were obtained with the $\frac{1}{50}$th. This was the maximum performance of the $\frac{1}{8}$th and readily bore amplification to 19,050 diameters. The field thus obtained with the $\frac{1}{8}$th, over seven inches in diameter, was absolutely flat. I send you herewith albumen prints of both sets of pictures. You will observe that the small pictures with the $\frac{1}{50}$th are the sharpest, owing in our opinion to somewhat better chemistry in making the negative, while, of the enlargements, that from the $\frac{1}{8}$th picture is best, owing to the greater flatness of the field in the original negative.

Without going into a discussion of the comparative merits of Powell and Lealand's $\frac{1}{50}$th in this place, it is interesting to observe that these photographs confirm the opinion expressed by Prof. Rood in this Journal[4] as to the circular nature of the markings on *Pleurosigma angulatum*, an opinion which had previously been expressed by Mr. Wenham.

At the date of publication of Circular No. 6, Surgeon General's Office, both Dr. Curtis and myself believed these markings to be hexagonal, as was stated and figured on page 148 of that work. The greater power now obtained has corrected this opinion, but it is worthy of note that in the present pictures the markings appear hexagonal in both the small ones, if viewed with the eye at the visual distance, while on close inspection or with a lens they are seen to be circular. In the pictures with 19,050 diameters the circular shape of the markings is very plain, but if viewed from a considerable distance or with a concave lens, they appear hexagonal. I also send you herewith a photograph of cartilage magnified 370 diameters, in illustration of the results attainable in the photography of the soft tissues. This

[4] On the evidence furnished by Photography as to the nature of the markings on the Pleurosigma angulatum; by Prof. O. N. Rood, this Journal, vol. xxxii, p. 335.

picture shows capsules, corpuscles and nuclei with the utmost sharpness.

In short, it is our opinion that henceforward photography is indispensable to the proper representation of microscopic objects, and is, as practised in the Army Medical Museum, even in its present condition, adequate to the satisfactory representation of all microscopic objects that do not depend for their value on colors.

ART. XXVII.—*Note on a Regular Dimerous Flower of Cypripedium candidum;* by ASA GRAY.

Mr. J. A. Paine, Jr., of New York, who two years ago detected an interesting monstrosity of *Pogonia ophioglossoides*, has now brought to me, preserved in spirit, a monstrous blossom of *Cypripedium candidum*, which demands a record.

The plant bears two flowers: the axillary one is normal; the terminal one exhibits the following peculiarities. The lower part of the bract forms a sheath which encloses the ovary. The labellum is wanting; and there are two sterile stamens, the supernumerary one being opposite the other, i. e., on the side of the style where the labellum belongs. Accordingly the first impression would be that the labellum is here transformed into a sterile stamen. The latter, however, agrees with the normal sterile stamen in its insertion as well as in shape, being equally adnate to the base of the style. Moreover the anteposed sepal is exactly like the other, has a good midrib and an entire point. As the two sterile stamens are anteposed to the two sepals, so are the two fertile stamens to the two petals, and the latter are adnate to the style a little higher than the former. The style is longer than usual, is straight and erect; the broad, disciform stigma therefore faces upwards; it is oval and symmetrical, and a light groove across its middle shows it to be dimerous. The placentæ, accordingly, are only two. The groove on the stigma and the placentæ are in line with the fertile stamens.

Here, therefore, is a symmetrical and complete, regular, but dimerous orchideous flower, the first verticil of stamens not antheriferous, the second antheriferous, the carpels alternate with these; and here we have clear (and perhaps the first direct) demonstration that the orchideous type of flower has two stamineal verticils, as Brown always insisted.

ART. XXVIII.—*Contributions from the Sheffield Laboratory of Yale College.*—XII. *Analysis of a Mineral Water;* by FREDERICK F. THOMAS, Ph.B.

THE mineral spring, the water of which is the subject of this notice, is situated in the town of Barton, Tioga Co., New York, about seven miles northeast of the village of Waverly, near what is called Talmadge Hill. It is one of two sulphur springs that have been observed in that county. The other, resembling it in character, is about twenty miles north, near the village of Spencer; both have been noticed in the State geological reports, and have been in repute for many years among the inhabitants of that region on account of certain remedial properties which their waters are supposed to possess.

This spring rises from rocks of the Devonian age—the blue argillaceous shale and sandstone of the Chemung group—of which many outcroppings may be seen in the immediate neighborhood of the spring. These rocks, according to examinations made in this Laboratory, consist principally of silicate of alumina, but also contain sulphuric acid, lime and magnesia in considerable quantities, some potash, soda and iron, as well as chlorine, organic matter, and a trace of manganese. They turn black upon heating before the blowpipe, but no effervescence is observed on treating them, in the pulverized state, with acids. The water partakes considerably in its mineral character of the properties of the rocks at the surface, but contains a very small amount of sulphates, while a large percentage of its mineral constituents are carbonates. The mean of two corresponding determinations of sulphuric acid made on the water as soon as received at the laboratory, gave 0·116 grain per gallon, while other estimations, made from water which had remained sealed in bottles for some weeks, gave a slightly larger amount, which resulted from oxydation of sulphur.

As the rocks in which the spring rises contain traces of manganese, that substance would probably be found in the water, were a large amount concentrated. None was however detected in the quantity of water at disposal.

The sulphur and sulphuretted hydrogen in the water may be due to the reduction of sulphates by organic matter and the subquent liberation of sulphuretted hydrogen by free carbonic acid.

The comparatively small amount of sulphates in the water, the presence of organic matter in the rocks, the temperature of the water, which is quite low, would be in support of this view; as well as the fact that none of the surface rocks appear to contain sulphids—quite a large excavation having been made in the immediate vicinity without revealing any pyrites.

A few feet from this spring there rises another, apparently from the same formation, but containing no sulphur. The bottom and sides of the spring are covered with a yellowish-white coating of separated sulphur; bubbles of gas rise at frequent intervals, and on reaching the surface burst, emitting the odor of sulphuretted hydrogen.

The substances contained in the water are potash, soda, ammonia, lime, magnesia, iron, alumina, carbonic acid, chlorine, sulphuric acid, sulphydric acid, silicic acid, organic matter, and slight traces of nitric and phosphoric acids.

The results of analysis were as follows:

Potash,	0·070	grains per	gallon.
Soda,	7·588	" "	"
Ammonia (NH_4O),	3·765	" "	"
Lime,	2·125	" "	"
Magnesia,	0·946	" "	"
Oxyd of iron and alumina,	0·360	" "	"
Carbonic acid,	12·992	" "	"
Silica,	0·983	" "	"
Chlorine,	1·293	" "	"
Sulphuric acid,	0·116	" "	"
Organic matter,	1·160	" "	"
Sulphur,	1·524	" "	"
Total,	32·910	" "	"

These substances may be combined in the following manner:

Chlorid of sodium,	2·045	grains per	gallon.
" potassium,	0·110	" "	"
Carbonate of soda,	11·119	" "	"
" ammonia,	6·950	" "	"
" lime,	3·650	" "	"
" magnesia,	1·987	" "	"
Sulphate of lime,	0·197	" "	"
Oxyd of iron and alumina,	0·360	" "	"
Silica,	0·983	" "	"
Organic matter,	1·160	" "	"
Sulphur,	1·524	" "	"
Carbonic acid,	2·626	" "	"
Total,	32·711	" "	"
Add oxygen equiv. to chlorine,	0·199	" "	"
	32·910		

The iron and alumina are given as they were present in the residue dried at 356° F. In the water fresh from the spring they appear to be in solution as organic salts. The amount of CO_2 uncombined in the above statement is almost precisely sufficient to convert the carbonates of lime and magnesia into bicarbonates (2·566 grains per gallon being necessary for the conversion of

the alkaline earths present into bicarbonates). The sulphur exists partly as sulphuretted hydrogen, and partly as alkaline sulphid. The results on carbonic acid and sulphur are too low since the gases were not examined at the spring. The total amount of sulphur was determined by oxydizing to sulphuric acid with aid of chlorine gas. From the total amount of sulphur, that amount corresponding to the sulphuric acid found by analysis was deducted and the remainder set down as sulphur. The carbonic acid was determined by adding weighed amounts of the water to chlorid of calcium that had been freed previously from carbonic acid.

The ammonia was determined by Boussingault's method. In general, the methods given in Fresenius's quantitative analysis were followed in the several determinations.

ART. XXIX.—*On the Nature of the Action of Light upon Iodid of Silver;* by M. CAREY LEA, Philadelphia.[1]

MUCH difference of opinion has long existed in respect to the explanation of certain phenomena of photographic action. In the vast majority of cases the action of light is a reducing one. Salts of iron, of uranium, and of other metals are reduced from a higher to a lower stage of oxydation, and the same is the case with the combinations of certain metallic acids, such as bichromates and tungstates. These phenomena present no difficulty. It is only when we come to the silver haloids that obscurity commences.

It is generally held, and there seems no reason to doubt it, that chlorid and bromid of silver undergo reduction when exposed to light. I shall therefore pass over these compounds, and discuss only the action of light upon the iodid.

In respect to this, two opposite opinions have divided those chemists who have seriously occupied themselves with the subject. Some believe the action of light on the iodid to be purely physical, others hold it to be connected with an absolute chemical change; some again holding this chemical change to be a reduction to a sub-iodid, others to metallic silver.

There can be no doubt that when iodid of silver is exposed to light in the presence of free nitrate of silver, it undergoes reduction. An examination of this reduced substance showed that it still contained iodine; when treated with nitric acid, a solution of nitrate of silver was obtained, with a product of yellow

[1] In the following series of investigations it has been the object of the writer to endeavor to fix, with greater exactness, the obscure chemical and physical phenomena which form the basis of the photography of the day. The details have been published in the journals especially devoted to photography. It has been suggested to him to make a brief abstract of these studies in their essentially chemical and physical relations.

iodid of silver. If the iodid of silver, after exposure to light in the presence of free nitrate, is carefully washed, the free nitrate is thereby removed. Digestion with hyposulphite of soda removes all neutral iodid present, and the residue consists wholly of that portion of the iodid that has been altered by the light. If this altered substance were metallic silver, it would of course dissolve wholly in nitric acid. But as just said, this it does not do, but leaves behind yellow iodid of silver perfectly soluble in hyposulphite of soda. It is clear, therefore, that iodid of silver is reduced by the action of light, when free nitrate of silver is present, to sub-iodid. This sub-iodid is converted by nitric acid into nitrate of silver and iodid of silver. And I have found the same to be the case when *tannic acid* is substituted for free nitrate of silver, though the action is greatly slower: as in the former case a sub-iodid is formed.

There is a question, however, far more difficult than these, to answer, and it is this: Does reduction of some sort invariably accompany the action of light upon iodid of silver? Is, or is not that action, in its essence a chemical action?

Before proceeding to investigate that question, another presents itself, demanding solution. It had been long held as an indisputable fact, which none had attempted to controvert, that perfectly pure iodid of silver was insensitive to light, and that sensitiveness only appeared when free nitrate of silver, tannic acid, or other "sensitizer" was present.

I soon satisfied myself that this asserted fact (for the explanation of which long discussions had taken place) had no existence whatever, and that pure iodid of silver was always sensitive to light. The long series of experiments, made with the most careful and multiplied precautions, and varied in many different ways, need scarcely be detailed here, except to say that I finally adopted, as most free from opening for cavil, the method of producing specular films of pure metallic silver on plates of glass: these were thoroughly iodized by very prolonged exposure to solutions of iodine, then thoroughly washed, and on these, invisible impressions made by light were developed without difficulty. The demonstration was so convincing that I have had the pleasure of seeing those who were the most earnest supporters of the old view, abandon it entirely.

I return then to the main question, which is: Does chemical decomposition necessarily accompany the production of an impression upon iodid of silver? In my opinion *it does not.* I hold that: 1. When perfectly pure iodid of silver, isolated, is exposed to light, it receives a physical impression only. 2. But that when certain other substances, for example, nitrate of silver, tannic acid, and perhaps many others, are present, then a chemical action, a reduction, does, or may, take place.

The second of these propositions is generally admitted; the first, on the contrary, is much contested. I shall therefore briefly state the results of the several series of investigations undertaken to arrive at a clearer view of the principles involved.

When pure iodid of silver, isolated, is exposed to light for a very brief period, an invisible, or latent image is produced, which by the action of a solution just ready to precipitate metallic silver, becomes evident.

If the action of light in producing an invisible image upon pure iodid of silver isolated be a chemical one, it is not possible that it should be destroyed except by chemical means.

A piece of glass supporting a film of pure iodid of silver isolated from all other substances was exposed for many hours to a strong sunlight. It was then placed in a dark closet for thirty-six hours, at the end of which time it was placed under a negative and exposed to light for two seconds. On pouring a developer over it, a clear bright picture instantly appeared. Thus the action of the sun for many hours had produced an impression which completely disappeared in thirty-six hours.

Now if the action of light is to reduce iodid to sub-iodid, how did this sub-iodid recover its lost proportion of iodine? The fact that the iodid was much more powerfully affected by a recent exposure of two seconds than by one which though thirty-six hours old, was many thousand times as long, and in light much more intense, seems fatal to the chemical theory. That theory holds that the production of a latent image is accompanied by a reduction. The plate in question then either should under the action of the developer have received a deposit all over, or else must have recovered its iodine. The latter case is not supposable, the other alternative did not take place, therefore the action of light could not have been chemical.[2]

In some cases, the action of light upon perfectly pure iodid of silver, isolated from all other bodies, may produce a visible

[2] The only objection to this argument that I have seen made is one by Dr. Vogel, who remarks that in one case when actual chemical decomposition undoubtedly does take place, sensitiveness is recovered by repose in the dark. This is in the case of Obernetter's paper; paper imbued with perchlorid of iron and protochlorid of copper. Light reduces the iron salt, which again converts the copper salt to dichlorid; on applying a sulphocyanid a precipitate is produced on the parts altered by light. It is alleged that this capacity to precipitate disappears in a day. It is a sufficient answer that iron and copper are highly oxydable metals, and that the action of the light upon the above salts, though undoubtedly chemical in its nature, *is rapidly undone by the action of the air*, which, as is well known, quickly converts protochlorid of iron into oxychlorid, and dichlorid of copper into a green mixture of oxychlorid and cupric chlorid. This explanation—evidently the correct one in the case of the iron and copper salts—cannot be applied to a metal like silver which has no tendency to oxydize by atmospheric action. Even if this answer were not in itself sufficient, it is further to be remarked that there is no analogy in the two developments—the silver development being accompanied by no decomposition of the image, as is the case with the iron and copper mixture.

image. This very remarkable fact, a description of which I published early in June last, and which has been since verified by other observers,[3] might seem to be inimical to the physical theory. A careful study proved the contrary. Pure iodid of silver, still moist, in considerable quantity, freshly precipitated and washed, was placed in a porcelain basin and exposed to sunlight. It instantly showed the slight darkening above referred to, but even after the action of the light and direct sunlight was continued for several hours, no reduced silver could be detected. The iodid after exposure was treated with dilute nitric acid, which restored the iodid to its bright yellow color. Now had this been effected by the action of the acid upon any sub-iodid formed by the action of light, then some portion of silver should have dissolved in the nitric acid. None however could be detected in it. It appears therefore that the effect of light upon pure iodid of silver, isolated from all bodies that can modify its action, is not a chemical one, whether its effect be visible or invisible, and that even when the action of light is prolonged to many thousand times the period sufficient for the production of a developable image, still no chemical alteration can be detected in the exposed iodid.

In addition to the foregoing, which are direct arguments, is to be placed the following indirect.

Believing that the action of light upon isolated pure iodid of silver is purely physical or molecular, it occurred to me to try whether another form of physical action, viz. *simple mechanical pressure*, could not be made to afford a basis for development. These experiments resulted in the formation of those curious effects which may be called *pressure images*. If a sensitive plate, ready for the camera, be pressed upon in any way, without removal from the dark room, and then be exposed to the action of a developing solution, all the parts so pressed upon will receive a heavier deposit of silver than the rest. This fact gives rise to various curious experiments.

If, for example, a piece of wood with lettering or figures cut out of it in open work be pressed forcibly on the sensitive film for a few seconds, and the plate then be developed the image of the lettering or open work will come out distinctly.

So if an embossed card be used, the embossed work will appear upon the film as soon as the developer has produced a deposit. The experiment may be varied in many ways: the material, so long as it is one which does not act chemically upon silver solutions, is unimportant; all that is required is a difference of pressure.[4]

[3] See paper by Major Russell in Sutton's Notes, vol. xi, No. 245.

[4] These experiments have been since repeated by Girard with substitution of glass and rock crystal, with like results.

Chemists are very familiar with the fact that in cases of slow precipitation, the precipitate tends to follow the path which the glass rod used to stir the solution, has travelled over. The analogy which this and the development of a latent image present cannot be missed, and they are brought closer still by the pressure images just described. In all cases a molecular alteration takes place, and the molecules thus altered seem to have their attraction for exterior objects increased, and for each other and the surrounding homologous particles diminished.

This has been beautifully illustrated by some experiments lately published. A steel burnisher being drawn firmly over a plate of glass, yet so as not to scratch it or abrade the surface, the glass on examination by polarized light showed its structure altered by a line of color where the burnisher passed. This molecular effect slowly diminishes, and in a few days the particles return to their wonted state.

The foregoing experiment beautifully illustrates the changes of molecular condition which bodies are capable of undergoing. I have just remarked, that by drawing a blunt glass rod over the inside of a glass vessel, the parts so treated attract to themselves the particles of falling precipitates in a very striking manner. Often if a glass beaker containing a solution has been much stirred before a precipitate takes place, that precipitate will search out and develop, so to speak, all the invisible lines which were the path of the glass rod. This development of these invisible lines has a very striking analogy with the development of a latent photographic image, and the experiment just cited, reveals by polarized light a change of structure in those paths over which the body has been drawn. In the course of hours or days, this molecular change gradually passes away, and the body recovers its original condition, just as I have shown that when as in the case of pure iodid of silver, isolated, no reduction takes place, the effect of light gradually passes off, leaving the iodid free to receive a new impression.

It seems almost superfluous to say that what is here stated of the development of pressure images is not given by way of explanation, but as illustration and confirmation of the general view expressed.

Before concluding, a few words seem required as to what explanation is to be given to the ordinary process of Negative Photography.

In the foregoing I have reasoned on the action of light upon pure iodid of silver isolated. In the camera, light is made to act upon iodid of silver in a very different condition. It is in contact with nitrate of silver and with organic matter, and here bromid of silver is present.

I think it is evident from what has been already said, that in this case *several* images are formed, superposed as it were, on each other,—First, a physical image upon the iodid of silver. Secondly, if the exposure be sufficient, an image formed of sub-iodid of silver produced by the action of light upon the iodid and nitrate. Thirdly, there may be an image formed by the action of light upon the silver or its iodid in connection with the organic matter of the film (collodion or albumen). And, fourthly, if bromid or chlorid be present in the film, these may undergo reduction.

The separate nature of these images, or some of them, which may exist in the same film, interpenetrating each other, may be shown by the following experiment.

If an ordinary bromo-iodized collodion plate be exposed in the camera for the ordinary time and then be thrown into a dilute solution of acid pernitrate of mercury, the whole *of the iodid and bromid will be dissolved*, leaving a film just as clear and transparent as the glass on which it rests. Let now this be well washed, and then a developer of silver and reducing agent be applied in the ordinary way. An image will at once start out, which by care and redevelopment may be brought up to any strength desired. Now the acid nitrate of mercury has the property of quickly dissolving not only iodid and bromid of silver, but also sub-iodid and metallic silver. Consequently whatever basis was afforded by the first, second, and fourth sources as above enumerated, was completely removed. Nothing remained but the third, and it is not improbable that in some of the "dry processes" this third source is the principal basis of the picture, though evidently only a subsidiary one, in the ordinary "wet process."

Another form of this experiment consists in developing the picture *before* immersing it in the solution of acid nitrate, and then in leaving it but a short time in that solution, so that only the visible picture shall be removed, and the film of iodid and bromid be left.

Here all basis for development depending upon reduction is removed, and the production of the picture must depend wholly upon the first and third of the foregoing causes to the exclusion of the second and fourth.

Without having extended my observations to the *Daguerreotype*, I may remark that this process must be ranked along with those in which there is a reducing substance present, and therefore reduction may take place. The metallic back of the daguerreotype plate stands in the same relation to the iodid that tannin and other so called sensitizers do. It seems probable therefore that in the case of the daguerreotype, there may be

sub-iodid of silver produced, and this seems to correspond with Hunt's results, and those of other experimenters since his time.

In fact I have succeeded in showing the analogy between these processes by a new experiment which seems to establish it in a very striking manner. I impressed a wet collodion plate in the camera and then treated it with a pyrogallic solution in which proto-nitrate of mercury was substituted for nitrate of silver. The reducing tendency of the pyrogallic acid predisposes the mercury to precipitate, just as it does the silver, and this precipitate is attracted to the modified iodid, precisely as the precipitation of silver. The result is the development of a picture on iodid of silver by mercury *in the wet way:* this is precisely the intermediate stage between the ordinary collodion process in which a plate is developed by silver in the wet way, and the daguerreotype in which it is developed by mercury in the dry way.

This experiment, though affording no direct proof that the ordinary impression of light is a physical one, still tends to confirm that view. For if the chemical theory be true, it is a reduced image that attracts the precipitate of silver, and such an image would not be likely to attract a foreign metal. But if we conceive the phenomenon to be altogether a physical one, the dissimilarity of the precipitate from the surface on which it falls, imposes no more difficulty than the dissimilarity of the nature of the glass from that of the precipitate which is attached to those parts of the glass which have been pressed by the rod in stirring.

In fact it may be truly said that whenever one body is pressed against another, the particles of the body pressed against tend to have their attraction for each other diminished, and their attraction for external bodies, whether homogeneous or heterogeneous, increased.

I have endeavored in this brief review of a subject replete with difficulties, to show that the action of light upon pure iodid of silver isolated cannot be a chemical reduction:

1st. Because that effect, even when carried many hundred thousand times further than in the ordinary photographic processes, perfectly disappears in a few hours, spontaneously, under circumstances which render it impossible to suppose that iodine could have been restored to replace that which (had reduction taken place) must have been disengaged.

2d. Because, even where the action of light is prolonged many hundred thousand fold the ordinary time, no reduced silver nor sub-iodid can be detected as present.

3d. I have shown that another metal, mercury, is capable of developing these images as well as silver.

4th. I have endeavored to show that a purely physical cause, to wit, mechanical pressure, is capable of producing a developable impression, thereby answering the objection of the inadequacy of a physical influence to create a basis of development.

And finally, I may remark that although the chemical theory is supported by some distinguished chemists of the present day, I am not aware that there is a single well verified experiment which can be brought forward in support of that view. Had there been, I should have submitted it to a most careful study, my object being to arrive at the truth, not to support a theory. In the absence of such, I have been necessarily obliged here to confine myself to the affirmative side of the question, in support of the existence of a physical image, distinct from chemical reduction, and though often accompanied by it, yet never necessarily.

I cannot conclude these remarks without expressing my thanks for the kind assistance given me by my friend Mr. Thomas P. Shepard of Providence, Rhode Island.

Philadelphia, July 10, 1866.

ART. XXX.—*Observations on the origin of some of the Earth's Features;* by JAMES D. DANA.[1]

THE work by Mr. Vose on Orographic Geology, briefly noticed in our last number,[2] closes its general review of the different hypotheses with regard to the origin of the earth's features, with a brief statement of "conclusions." Among these conclusions are the following: that the deposition and subsidence of large accumulations of sediment produce the folding and compression of strata; that the alteration or metamorphism of sediments has arisen primarily from compression, "this force being resolvable into other actions that give rise to phenomena which seem to be due to heat and to chemical action;" and that "it is better philosophy" to attribute the results of metamorphism to this cause "than to a supposed central mass of fluid, gaseous emanations, and the like, that we know nothing about, which seem opposed by important facts, and which, from all we know, should act generally and not locally; and especially not in the regions of great accumulation which from their very thickness would seem to be most removed from any source of heat beneath the earth's crust." (p. 133.)

[1] For the writer's earlier discussions of this subject, see volumes ii, iii, iv, vii, xxii of this Journal, second series, the writer's Expl. Exp. Geological Report, and his Manual of Geology.

[2] Page 123, of this volume.

In thus holding that the "plication of strata has resulted from the subsiding of large masses of sediment" (p. 134), the author follows Professor James Hall, an abstract of whose views, with some criticisms, he gives on pages 47 to 55. The adoption of this hypothesis by an author treating professedly of geological dynamics prompts to an early consideration of its merits; and I propose to state in this place some of the objections that have occurred to me. These objections are in part alluded to by Mr. Vose, yet without allowing them their full weight.[3] The method also of metamorphism differs but little from that of Prof. Hall.

As Mr. Hall's views have not been explained in this Journal, I here cite a few paragraphs from the convenient abstract of them given by Mr. Vose, with others from the full exposition of the subject by Mr. Hall in the Introduction to the third volume of his Paleontology of New York.

"When large masses of sediment are spread along a sea-bottom, as originally along the line of the Appalachians, the first effect will be a yielding of the earth's crust beneath, and a gradual subsidence. We have evidence of this, first in the great amount of material accumulated; for we cannot suppose that the sea was originally as deep as the thickness of these beds: indeed, the ripple-marks, the marine plants, &c., prove that the sea in which these deposits were successively made was always shallow. The accumulation could thus only have been made by a gradual subsidence of the ocean-bed. The greatest depression would be along the line of the greatest accumulation; and, in the direction of the thinning margin, the settling would be less. By this process, as the lower side became gradually curved, rents and fractures upon that side would occur, while the compressed upper surface would be wrinkled and folded. The sinking down of the mass produces a great synclinal axis; and within this, whether on a large or a small scale, will be numerous smaller synclinal and anticlinal axes. And the same is true of every synclinal axis: it will contain still smaller synclinals, or, if the folding is not sharp enough to actually make the smaller folds, the tendency will still be to produce the various phenomena of compression (cleavage, &c.). That the subsidence was periodical, we have the best evidence in the unconformability of certain beds; showing that the lower one was bent and disturbed before the deposition of the upper one. This hypothesis explains why the mountain elevations in disturbed regions bear in their altitude a much less proportion to the thickness of the formations, than the hills in undisturbed regions do to the thickness of their beds. The great weight of the thicher beds has caused them to subside so much, that a larger part of their depth is now below the level of the sea."—*Vose*, pp. 48, 49.

"The direction of the waves formed by the settling of any wide area will be parallel to the great synclinal axis; a fact already stated in different language by Professor Rogers. The present mountain ranges are the evidences of the ancient oceanic currents, and thus the causes which determined the elevations existed long before the production of the moun-

[3] See his note to page 48, and pages 53, 54.

tains themselves. At no point, says Mr. Hall, between the Appalachians and the Rocky Mountains, could a mountain chain have been produced, because the accumulated materials were insufficient. The Rocky Mountains owe their greater height to a series of later deposits than those which cap the Appalachians; the White Mountains are covered with a later formation than that which covers the Green Mountains; the Alps are newer than the Jura; and, generally, if it is the original deposition of the materials that has produced mountains, then the greater the accumulation the higher will be the chain when it is finished."—*Vose*, p. 51.

"It nowhere appears that this folding or plication has contributed to the altitude of the mountains: on the other hand, as I think can be shown, the more extreme the plication, the more it will conduce to the general degradation of the mass, whenever subjected to denuding agencies." "The elevation [of the Appalachians] has been of continental, and not of local origin." "I believe, moreover, that this mountain chain, in its component parts, and in its mode of accumulation, and the process by which it has assumed its present position does not differ materially from other mountain ranges." If the fundamental rocks of the Alps are of Paleozoic age, and the sequence has been continued, even with some interruptions, to the end of the Jurassic period or later, it is no wonder that there are high summits, for the accumulation must have been enormous; and if to the Liassic and Jurassic we add the Cretaceous and Tertiary, we may get mountains of the elevation of the Himalayas. For I hold that no mountains of this elevation can occur without the long-continued accumulation of sediments; sediments not simply marking this altitude, but vastly more, for there is doubtless as much of the mass below the level of the sea as above it. This view we find applicable to the Appalachians, and it must be a necessary condition of mountain elevation."—*Hall's Pal. N.Y., Vol. III, Introduction.*

"We must look to some other agency than heat for the production of the phenomena [of metamorphism]; and it seems that the prime cause must have existed within the material itself, and that the entire change is due to motion, or fermentation, and pressure, aided by a moderate increase of temperature" produced by the sinking of the thickened mass to a level where the surrounding temperature was higher.—*Ibid.*, p. 77.

The hypothesis then is that the thickening of the deposits along the Appalachian region to 40,000 feet, more or less, which went on through the Paleozoic ages (and which was due mainly, as Mr. Hall holds, to material distributed by the northeastern oceanic current, now the cold Labrador current) ultimated in a subsidence of 40,000 feet, and in flexures, plications, fractures, metamorphism, trap dikes, and mountains; and that the same general principle was exemplified in the origin of the Rocky mountains and Andes, the Alps and Himalayas, and all other mountain elevations.

A. The fact of a subsidence in the Appalachian region accompanying the accumulation of the deposits is established by shallow-water markings in most of the successive strata, from the bottom to the top of the Paleozoic, as stated by Professor

Hall. And it must be admitted, further, that in other regions of the globe subsidence has, in most cases, attended similar accumulations. We do not question this postulate of Mr. Hall's. The Carboniferous formation of Nova Scotia is a case of the kind where the evidence is clear; for although 16,000 feet thick, it bears throughout proofs of its origin near the ocean's level, in dirt beds, coal seams, estuary deposits, and the like.[4] The 16,000 feet of thickness prove, therefore, 16,000 feet, approximately, of gradually progressing subsidence.

The subsidence connected with the formation of the successive thick deposits of sediments in the Appalachian region was, then, *a foot-per-foot movement;* that is, taking into view the grand result through the Paleozoic, there was a foot of sinking for a foot of accumulation. The sinking may have gone on paroxysmally, or with intervals of quiet, and of reversed oscillation, and unquestionably did so; and still it was, as a whole, for the long series of periods and ages, a foot-per-foot movement,—the whole amount of subsidence being gradually accomplished, and equalling at least the mean thickness of the deposits.

Now such easy sinking from mere gravity would require—

(1) A very yielding crust; and, therefore, a very thin one; with

(2) A perfectly mobile liquid beneath it. In fact, this foot-per-foot movement would demand that

(3) There should be no impediment, either in the crust, or in the density of the fluid below, to subsidence from added weight, and, at the most, only temporary checks to the movement.

Considering, then, the nature of rock, the laws of resistance, the form of the earth, the dependence of sinking in a floating body on its density as compared with that of the liquid; and further, that the sediment brought in *displaces its volume of water*, so that the weight it adds is only the difference of the two; what should be the thickness of the earth's rocky crust, and what its density, in order that it should be thus sensitive to the touch of sediments? Could there be the foot-per-foot movement under any degree of thinness? Surely the 800 miles of the mathematicians, or the densely viscid or the solid interior of some theorists, would give it no chance. And would a thickness of twenty miles, or of ten, or of five, allow of this no-resistance movement? The idea is obviously opposed to the very nature of the earth and its forces.

But suppose this subsidence through gravity to take place. It would occasion (1) some lateral pressure in the subsiding crust, which would react upon the crust around, tending to produce displacements where the crust was thinnest and weakest; and (2) pressure upon the interior liquid mass of the globe, which would be felt variously over the whole inner surface of the shell.

[4] For details on this point, see Dr. Dawson's paper in the Jour. Geol. Soc. of London, No. 86 (May, 1866), p. 95.

The effects would, hence, be widely distributed, and be but feebly appreciable in any region, as will be seen on plotting to the scale of nature. A small part only of the action would tend to cause displacements over the region of the *thickened* (and thereby strengthened) sinking crust. Flexures on the scale of magnitude and number presented in the Appalachian region, grouped, as they are, most thickly over its middle and to the eastward, and fading out westward where the crust *has not one-fourth as great a thickness of sediments*, are opposed to the fundamental principle appealed to in the hypothesis.

We observe, further, that the larger part, at least, of the bold flexing of the Appalachian rocks took place toward, or at, the close of the Carboniferous age, *after* the Paleozoic deposits of Pennsylvania had been laid down; while, on the contrary, it should have very largely attended its progress, if sinking were the cause, since seven-eighths of this sinking had taken place before the era of the Coal-measures. Moreover, the metamorphism and the making of the mountains were mainly involved in the grand final result, and were a part of it.

We may well question whether the earth, as long as its crust was so sensitive to the weight of a layer of gravel, would anywhere be able to hold up mountains; for mountains have gravity as well as gravel beds or other sediments. We should hardly have expected, after a sinking had been going on in the Appalachian region for ages through simple gravity, a foot for a foot of deposits, that there, on that same yielding crust, the Appalachian mountains should have found a firm standing place, and especially before the vast Rocky mountain region, half the continent in breadth, was through with its sinking process; or that in Triassic and Jurassic times, the Green mountains should have kept their place on the west, and the high table lands on the east, when the crust was so weak below that the sands washed into the Connecticut valley by the hillside streamlets caused it to bend downward an inch for every inch of accumulation, till some thousands of feet of sandstone and of subsidence had been produced;[5] or that the more ancient ridges of the Alps should have been able to stand with uplifted heads while great Tertiary basins in Switzerland, and over the regions of the present Apennines and Pyrenees, and in other parts of Europe, were so thinly bottomed that a sinking, through the weight of gravel of no greater specific gravity than the rocks of the Alps, was going on by the thousands of feet; and that these same sinking basins should have next become the site of mountain peaks.

B. It has been remarked above that the hypothesis of Mr.

[5] These Triassico-Jurassic beds of the Connecticut valley have a width of 20 miles or so, and extend from Long Island Sound at New Haven 120 miles to northern Massachusetts. Prof. Hall includes the region among the examples of subsidence from thickening accumulations of sediment. (See Paleontol. N. Y., vol. iii.)

Hall necessarily presupposes an exceedingly thin crust over the liquid interior. And yet the advocates of this hypothesis have for the most part rejected the idea that the cause of metamorphism lies, to any great degree, in the heat of this liquid interior. It is obvious, that with so thin a crust as the gravity-hypothesis demands (supposing any degree of thinness to meet its requirements) it would be superfluous to look to any other source for metamorphism, or for the material of trap dikes. It cannot be said of the central mass of fire by one advocating such an hypothesis, that it is a source of heat of which "we know nothing." A very thin shell over a sea of fire of extreme mobility is one of the essential conditions demanded by the hypothesis. Heat cannot come from *compression* occasioned by the slow action of gravity; for this produces none: and how can metamorphism?

C. Mr. Hall's hypothesis has its cause for subsidence, but none for the lifting of the thickened sunken crust into mountains. It is a theory for the origin of mountains, with the origin of mountains left out. Mr. Vose has some just criticisms on this point.

I do not now propose to present arguments in favor of the hypothesis which appeals to contraction from cooling as a cause of change of level. Although I have been inclined to give it the preference to others, I freely acknowledge that it has its difficulties. I would remark only on one point which may need an additional word of explanation.

All admit that the mountains of the globe are situated mostly along the border regions of the continents (taking these regions as 300 to 1000 miles or more in width), and that over these same areas the sedimentary deposits have, as a general thing, their greatest thickness. At first thought, it would seem almost incredible that the upliftings of mountains, whatever their mode of origin, should have taken place just where the earth's crust, through these sedimentary accumulations, was the thickest, and where, therefore, there was the greatest weight to be lifted. This difficulty Mr. Vose seems to regard as bearing especially against the hypothesis of contraction, while in reality it is the least of all obnoxious to it. Were the force causing elevation, under this agency, one acting directly *from beneath*, the gravity of the mass would prove an obstacle. But it is not so when the disturbing or uplifting force is *lateral* action or pressure from tension in the contracting crust. Under this agency the disturbances, and the mountain-making, find no great impediment in the thickness of the accumulations. Moreover, it is to be noted that the thick accumulations are produced just where oscillations and disturbances, or great yieldings in the crust, had been in progress through the long preceding ages (attending

the accumulation of the sediments), and, therefore, just where such disturbances or yieldings were most likely to continue to occur through after time. Earthquakes show that even now, in this last of the geological ages, the same border regions of the continents, although daily thickening from the sediments borne to the ocean by rivers, are the areas of the greatest and most frequent movements of the earth's crust.

ART. XXXI.—*Contribution to the Chemistry of the Mineral Springs of Onondaga, New York;* by CHARLES A. GOESSMANN, Ph.D., Chemist to the Salt Company of Onondaga.

SOME of the characteristics of the Brines of Onondaga, N. Y., have already been illustrated by a series of analyses in two previous reports elsewhere published.[1]

The following pages may be considered as a continuation of the discussion on subjects of similar import, since I propose to treat here of certain peculiar features of the locality where these brines are found.

The majority of the wells, from which the supply of brine for the Onondaga Salt Works has been hitherto obtained, are located in a half circle around the southern and eastern shores of Onondaga Lake, north and west of the city of Syracuse. This entire district consists mainly of low lands, which are yet partly in a marshy state. They have been reclaimed in the course of time from the original lake bed, by natural and artificial drainage, and extend from one to one and half miles south of the lake. They are everywhere bounded by more or less abruptly rising grounds. These embankments, toward the east and west, at the southern end of the lake basin terminate, in several places, in hills of a hundred feet and upward in height, consisting chiefly of sand and gravel. In other quite frequent instances may be seen the outcroppings of the red and blue shale of the Onondaga Salt Group at a considerable elevation above the level of the lake. The Onondaga red and blue shale, in a crumpled condition, forms a clay of corresponding color; its present outcrops seem to have been originally covered with the same diluvial detritus which is spread so extensively over the northwestern part of the State. The shales of the Onondaga Salt Group underlie, also, the lowlands at the southern termination of the lake, dipping apparently at a rapid rate toward the center of the valley or its proximity, and are supposed (Beck) to form a natural underground basin in which the brine accumulates. This pre-

[1] Report on the Brines of Onondaga, by C. A. Goessmann. Syracuse, N. Y., December 6, 1862.

Report on the Manufactory of Solar Salt, &c., by the same. Syracuse N. Y., December, 1863.

sumed basin is no doubt, filled in its lower portions with the same *debris* which covers the surrounding hills. Layers of sand, coarse gravel, loamy soil and hard pan are found here quite frequently, alternating with each other. Upon the deposit, within the low lands (mainly along the banks of the Onondaga Creek), rests a considerable amount of alluvial material of a loamy nature, sometimes rich in pebbles. Trunks of trees and hickory nuts, in a tolerable state of preservation, have been found at a depth of from twenty-five to thirty feet. These formations, particularly near the outcrops of the Onondaga shales, or at the termination of their surface drainage, are frequently found to be filled with an abundance of water of a peculiar saline character.

These waters sometimes contain only mere traces of chlorids, while in those from other similar localities in their vicinity a considerable amount of chlorid of sodium may be observed.

Among the questions to which these facts give rise, the following appeared to me of great interest:

First: Is there any relation between the chemical composition of the spring waters peculiar to the locality, and the brines?

Second: What chemical changes may result from their union, should their composition materially differ.

Third: Do the waters of the springs and the brines derive their characteristic qualities from soil or rocks of one and the same kind, though in different conditions; or do they both owe their peculiar chemical composition to entirely different sources; and if so, where are these sources located?

We may, for instance, suppose—adopting to some extent a view which has been advocated with much propriety in similar connections—that in the course of time an increased amount of percolation, or a certain order in the stratification, or a difference in the nature of the various layers, or particular advantages in drainage in general, may have favored a more rapid extraction of the more soluble compounds as chlorid of sodium, etc., in one locality than another. In fact, the well known and interesting discovery by A. Eaton,[2] of pseudomorphs of chlorid of sodium found in layers of a hardened clayish deposit—somewhat remote from the present brine-supplying district—contends strongly in favor of a previous gradual extraction of larger quantities of more soluble saline compounds (chlorid of sodium in particular) from the rocks and soil of the surrounding country.

The existing deficiency[3] of properly detailed geological records of numerous wells sunk at former periods is a deplorable fact, which seriously interferes with the discussions of the questions above proposed. For venturing at all, under such disad-

[2] W. Haidinger. Mittheilungen, etc., November 12, 1846. Wien. This Journal, xv. January, 1829.

[3] We are indebted to the Hon. George Geddes for a very valuable map of the outlines of the geology of Onondaga County.

vantageous circumstances, on the third question, I trust I may be justified by the course I shall adopt, viz: presenting merely a few facts which have come within my observation. Future local geological investigations must decide upon their value.

In entering upon considerations of the above questions I selected two springs from an elevation several miles distant from the brine-bearing district, two in its immediate vicinity and a sample of average brine of the Syracuse district.

The samples were as follows:

a, From a well at the north of a hill where, formerly, numbers of pseudomorphs of chlorid of sodium had been found; the well terminated in a hard clayish shale.

b, From a well situated midway between the former and the brine-furnishing locality; the well terminated in the red clay of the Onondaga Salt Group. Both wells were at least from forty to fifty feet above the level of the lake.

c, From a spring within the brine-producing district, at a height of about ten feet above the level of the lake.

d, From a spring in close proximity to *c*, and at the same elevation.

e, A brine from the vicinity of springs *c* and *d*.

a. Water from a well (47 feet deep) on Willow street near the corner of Catharine street, in the city of Syracuse.

The well from which this water was collected is situated in the vicinity of one of those spots (James street height) where, some years ago, while workmen were engaged in grading James street, a considerable number of pseudomorphs of chlorid of sodium were found. Serpentine was also discoverd not far off. Two prominent hills, of which the most conspicuous is known as "Prospect Hill," intervene between that locality and the quite abrupt descent of the high grounds around the east and southwest side of the former lake bed—our present brine-furnishing district. In sinking this well, (Nov., 1863,) a layer of gravel-bearing loamy soil of ten feet thick was passed, then twelve feet of crumbled green shale, and lastly twenty-five feet of a hard light green clayish shale. The water usually stands fourteen feet. The ground perforated by this well is probably fifty feet above the level of the lake, and about two and a half miles from the nearest salt well.

One thousand parts of this water contained—

Calcium, - - - -	0·2302 parts.
Magnesium, - - -	0·0359
Sodium, - - - -	0·0101
Silica, - - - -	0·0050
Chlorine, - - - -	0·0156
Sulphuric acid, - - -	0·3442
Free carbonic acid, - - -	(not determined)

1000 parts of this water left at 200° to 212° F., 0·8906 parts of solid residue; one gallon would consequently leave 3·36525 grams or 51·9849 grains.

b. Water taken from a well sunk from the top of Prospect Hill, to a depth of seventy-five feet, i. e., the level of Salina street at the corner of Lock street. Prospect Hill lies about midway between the well which furnished the water for Analysis *a*, and the northwestern termination of the high embankment (30 or 40 feet) around the eastern and southeastern shores of Onondaga Lake and its adjoining low lands. Prospect Hill consists mainly of gravel and is covered with numerous boulders, and underlaid with the red clay of the Onondaga Salt Group. The gravel is here and there interspersed with layers of cemented gravel (hard pan) and of a red loamy soil. These layers are of varying extent and apparently without any order of succession. The water subjected to analysis was taken from the first quantities drawn from the well soon after its completion (April, 1863). Quantitative tests for carbonic acid and iron, being under existing circumstances of no value, were omitted.

One thousand parts of this water contained—

Calcium, - - - -	0.52838
Magnesium, - - -	0·03954
Sodium, - - - -	0·00821
Sulphuric acid - - -	1·02660
Chlorine, - - - -	0·01268
Silica, - - - -	0·00450
Iron, - - - -	(not determined)
Free carbonic acid, - - -	" "

1000 parts of this water left, at 200° to 212° F., 2·0080 parts of solid residue. The residue from one gallon would therefore be 118·8602 grains.

c. This water was taken from a spring near the Syracuse Pump House, on the bank of Onondaga Creek, upon grounds which had at one time been occupied by vats for the manufacture of Solar Salt. The spring issues from an embankment near the place where the Onondaga Creek has forced its way through the southern enclosure of the low lands around the south shore of Onondaga Lake. This vicinity is rich in springs of similar character.

This peculiar water was found at a depth of from forty to fifty feet, where loamy soil or hard-pan had intervened between it and the brine proper. Brine had, no doubt, more or less access to some of these springs, if only by surface percolation. The spring which furnished me with the sample for this analysis was somewhat protected by a tight barrel in which it was purposely enclosed; and the barrel was several times emptied before collecting the sample for examination, (Sept., 1863).

One thousand parts of this water contained—

Calcium, - - - -	0·35265
Magnesium, - - -	0·07620
Sodium, - - - -	4·50454
Sulphuric acid, - - -	0·64379
Chlorine, - - - -	6·95266
Silica, - - - -	0·00490
Free carbonic acid, - - -	(not determined)
Carbonate of protoxyd of iron (traces),	" "
Bromine (traces), - - -	" "

1000 grams left, at 200° to 212° F., 13·0340 grams of solid residue; one gallon would consequently leave 49·2405 grams, or 760·7309 grains.

d. This sample of water was taken from a spring about twenty-five to thirty feet distant from the last one, *c.* The spring is enclosed in a tight wooden tank of 10 to 12 feet deep, and issues at the foot of an embankment from thirty to forty feet high. Its elevation above the level of the lake corresponds nearly with that of the spring *c.*

1000 parts of this water contained in solution—

Calcium, - - - -	0·28147
Magnesium, - - -	0·07700
Sodium, - - - -	4·01378
Silica and alumina, - -	0·01770
Sulphuric acid, - - -	0·48150
Chlorine, - - - -	5·30918
Bromine, - - - -	0·00232
Free carbonic acid, - - -	(not determined)
Carbonate of protoxyd of iron, -	" "

1000 parts of this water left, at from 200° to 212° F., 11·7382 parts of solid residue. One gallon would therefore leave 685·07402 grains of residue. This residue contained in combination, 0·1150 parts of carbonic acid.

e. The brine for this analysis was taken from a salt well in the vicinity of *c* and *d*, (July 30, 1863).

Calcium, - - - -	2·25005
Magnesium, - - -	0·36799
Sodium, - - - -	61·06500
Potassium, - - - -	0·05720
Iron, - - - -	0·02123
Chlorine, - - - -	96·36355
Bromine,[4] - - - -	0·02080
Sulphuric acid, - - -	3·39550
Free carbonic acid, - -	(not determined)

1000 grams of this brine contained 164·243 grams of saline

[4] Iodine, traces.

residue; one U. S. gallon[5] would therefore contain 9586·9891 grains.

The brines of Onondaga, though differing somewhat in concentration, vary but slightly in regard to the relative proportions of their component parts; and the analytical statement just made, will be found to meet all practical requirements.

In 1000 parts of	Willow st. well.	Prospect Hill well.	Mineral water.	Mineral water.	Syracuse brine.
are contained	*a.*	*b.*	*c.*	*d.*	*e.*
Calcium,	0·2302	0·5284	0·3526	0·2815	2·2500
Magnesium,	0·0359	0·0395	0·0762	0·0770	0·3679
Sodium,	0·0101	0·0082	4·5045	4·0137	61·0650
Potassium,*	....	....	....	....	0·0572
Sulphuric acid,	0·3442	1·0266	0·6437	0·4815	3·3955
Chlorine,	0·0156	0·0127	6·9526	6·3092	96·3635
Bromine,*	....	....	....	0·00232†	0·0208
Silica,*	0·0050	0·0045	0·0049	0·0177†	....
Carbonic acid,*	....	....	....	0·1150‡	....

* In all cases where no figures are given no quantitative tests have been made.
† Silica and alumina. ‡ Combined in the residue left at 200° to 212° F.

Although, unquestionably, much significance must be conceded to the fact that the same group of elements form the most prominent features of the analytical results, there nevertheless existed some doubts whether a mere succession of extractions of the same strata or the same kind of rocks, etc., would suffice to explain satisfactorily the peculiar nature of the various liquids; a view which appears still more conspicuous when these component parts are arranged in the combinations most likely to be present in each of the waters.

Having arrived at this point, I preferred to institute some inquiry in regard to the action of certain compounds toward each other under circumstances similar to those I should be obliged to take into consideration if a discussion of the second question should promise encouraging results; and in this connection I would call particular attention to the earlier statements of C. B. J. Karsten,[6] and the results given in the highly interesting publications of T. Sterry Hunt,[7]—the former treating mainly of the nature and changes of the brines—the latter more especially of the chemistry of natural waters in general.

My experiments were in some cases designed to give merely an idea of the degree of certain changes under given circumstances. It appeared to me of importance to ascertain—

[5] One United States gallon is equal to 241 cubic inches or 3778·625 grams or 58318 grains.

[6] C. B. J. Karsten, Salinenkunde, Berlin, 1843.

[7] T. Sterry Hunt, Chemistry of Natural Waters, this Journal, March, July and September, 1865.

I. *How does carbonate of magnesia act upon sulphate of lime in the presence of free carbonic acid?*—To test the degree of this action, I adopted the following course. I mixed in a suitable vessel ten grams of well washed carbonate of magnesia, twenty-five grams of sulphate of lime, (gypsum,) and five hundred cubic centimeters of distilled water; which mixture I afterwards treated for a short time each day during four weeks, with well washed carbonic acid gas, so as to secure a constant supply of bicarbonate of magnesia to the solution of gypsum. The solution thence resulting, after being separated by filtration from the white residue, was equal to four hundred and fifty cubic centimeters. I placed it in a flat glass dish, and left it to gradual evaporation at the ordinary temperature.

After a few days rest the bottom of the vessel began to be covered with a small amount of sediment. No further noteworthy change took place, until the whole liquid gradually formed into a solid crystalline mass, consisting in the main of a net-work of needles. The solution, being tested before its solidification, was of a slightly alkaline reaction; the crystals resulting from its evaporation were transparent, yet crumbled readily to a white powder when exposed to dry air.

One hundred parts of an apparently air-dried sample of the saline mass, contained—

Carbonic acid, - - -	1·6721
Sulphuric acid, - - -	31·4803
Calcium oxyd (lime), - - -	0·7988
Magnesium oxyd (magnesia), -	17·0327

One hundred parts of the same mass, heated to a dull red heat, lost 51·0593 parts of its weight; leaving therefore 48·9407 parts of a white residue.

These analytical results, if considered in connection with the general properties of the original liquid as above stated, and the consequent solid crystalline residue, tend to prove that the solid matter may, with some propriety, be considered as consisting of

Carbonate of lime, - - -	1·4268
Basic hydrated carbonate of magnesia $3(MgO, CO^2)+(MgO, HO)$, -	2·4911
Sulphate of magnesia (MgO, SO^3+7HO),	96·8020

As the whole amount of the saline compound obtained in the experiment just described, was 22·605 grams, we learn that about 14·5 grams of gypsum had been decomposed, while some of the carbonate of lime (or more properly, bi-carbonate) left in the originally diluted liquid, had been separated during the progress of its concentration as carbonate of lime. The apparently slow decomposition of gypsum must be attributed to the limited solubility of that compound. It is not unreasonable

to presume that carbonic acid, under pressure and at common temperature, would alter the degree of action above illustrated.

II. *How does carbonate of magnesia act upon sulphate of lime in the presence of free carbonic acid and chlorid of sodium?*—In this investigation I proceeded thus: I weighed into a glass bottle 31 parts of commercial carbonate of magnesia, 86 parts of gypsum, 58 parts of chlorid of sodium, and 3000 parts of distilled water, and treated the whole mass with carbonic acid gas for several weeks, as described in a former experiment. The filtrate, separated from the undissolved white mass, which mainly consisted of gypsum and carbonate of lime, besides mere traces of carbonate of magnesia, was evaporated to dryness at 180° F., and the residue subsequently extracted with absolute alcohol. The alcoholic extract contained (beside a very small quantity of chlorid of sodium), 0·4370 grams of chlorid of magnesium, while the residue left after the extraction by alcohol contained 0·0570 grams of oxyd of calcium, 0·2243 grams oxyd of magnesium, and 0·6860 grams of sulphuric acid. This proved the presence of 0·6532 grams sulphate of soda, 0·4773 grams sulphate of magnesia, beside 0·1018 grams carbonate of lime, and 0·1236 grams carbonate of magnesia, with part of the chlorid of sodium unchanged. These results indicate,

1. That gypsum, carbonate of magnesia, and carbonic acid in the presence of chlorid of sodium, form chlorid of magnesium, sulphate of soda, and carbonate of lime.

2. That at a certain higher temperature, the sulphate of soda and chlorid of magnesium partly re-transform into sulphate of magnesia and chlorid of sodium.

3. That the solubility of the gypsum governs the degree of decomposition.

(To be continued.)

ART. XXXII.—*A new Meteoric Iron, "the Colorado meteorite," from Russel Gulch, Gilpin Co., near Central City, Colorado Territory;* by J. LAWRENCE SMITH, Prof. Chem. in University of Louisville.

I HAVE known of the existence of a new meteoric iron from Russel Gulch in Colorado, for about two years, but it was only recently that it passed into my hands. I first heard of it in the possession of Mr. Fisher of New York, who subsequently turned it over to Prof. C. F. Chandler of Columbia College, New York, who kindly submitted it to me, as I am furnished with the necessary means for cutting up and scrutinizing thoroughly this class of bodies.

The mass of iron is accompanied with the following label: "Meteoric iron found in Russel Gulch, Feb. 18, 1863, by Mr. Otho Curtice. Weight 29 lbs. Brought to New York, Feb. 1864."

The mass measures in its extreme length, breadth, and thickness, $8\frac{1}{2}\times7\frac{1}{4}\times5\frac{1}{2}$ inches. It is perfect in all parts except at one extremity, and, as stated above, weighs 29 lbs.

This iron is one of medium hardness, with the density 7·72, and when cut through was found to contain a few small nodules of iron pyrites. It is attacked readily by nitric acid, and gives bold Widmannstättian figures without very sharp angles. It resists the action of the air and moisture very well, and is consequently but little altered on the surface. No siliceous minerals could be traced in any of the crevices. On analysis its composition was found to be—

Iron,	90·61
Nickel,	7·84
Cobalt,	·78
Copper,	minute quantity.
Phosphorus,	·02
	99·26

I have not made any further observations in relation to the presence of copper in meteoric iron since 1852, when I called attention to it. Since then I have become more confirmed in the opinion, then first expressed, that copper would be found in all meteoric irons; this has been the result of examinations of many well known meteoric irons, and all new ones that have come under my examination.

One or two grains of the iron is all that is necessary for the examination, if it be done carefully; but four to five grains had better be used. Dissolve the iron in chlorhydric acid, and if necessary add a little nitric acid; it is as well at all times to add a drop or two at the end of the operation. Evaporate away the excess of acid, add water, precipitate with sulphuretted hydrogen until there be an excess of gas in the solution; throw on a filter and wash with water containing a little HS, dry the filter, burn in a porcelain crucible, treat the residue with a little *nitromuriatic acid*, and evaporate to dryness, with the addition of a drop or two of sulphuric acid; treat the residue with water, when the introduction of a clean plate of iron will cause a deposition of the copper with all its characteristic properties.

ART. XXXIII.—*On Gay-Lussite from Nevada Territory;* by B. SILLIMAN.

IN September, 1864, I visited the body of saline water known as Little Salt Lake, situated near Ragtown, about a mile and a half south of the main emigrant road to Humboldt. It fills the bottom of a deep funnel-shaped depression in the Desert plain. The form and other peculiarities of this depression suggest a volcanic origin. It is distinctly crater-shaped, with the outline a double ellipse, made apparently by the coalesence of two craters; the larger is to the north, and has a diameter of about a mile and a half. The whole length north and south is somewhat greater than that from east to west in the larger division. The water-surface is about 200 feet below the lip of the crater, which is elevated somewhat above the general level of the plain. The slope of the converging sides is steep, varying from 25° to 45°; the approach to the water is therefore difficult, except at one or two points where an oblique footpath has been worn. There is a narrow margin or beach, varying from a few yards to a hundred feet or so, covered with shoal water, and the shore then plunges off to very deep water. There is a small island in the northern or larger division of this lake, also surrounded by shoal water. The section of the slope shows a series of beds of volcanic materials, lapilli and ashes, mixed with boulders or masses of black basalt, and concretions from thermal springs. The shores on the west side are also skirted with calcareous matter, and there is a steady flow of water from numerous small springs of fresh water into the lake. One of these springs is a copious fountain of excellent drinking water. The water of the lake is very saline. Its taste is salt, bitter, and decidedly alkaline. Its effect on the skin in bathing is that of a solution of an alkaline carbonate, and its odor is strongly marine. The rocks are encrusted with saline matters resulting from the evaporation of the waters.

The surface of the lake swarmed with small ducks; and divers sandpipers, cranes and other aquatic birds were on its shores. Myriads of larvæ of a species of fly (equally abundant at Mono lake) swarm in the shallow waters of the shore, but no fish appear to live in it. The water is so dense that a swimmer floats on it like a cork. There are no thermal springs now active in the lake, the temperature of which is normal.

Gay-Lussite.—A large stick floating on the shore and covered completely with an incrustation of yellowish-white crystals tubular in form attracted my attention. A careful search along the shores failed to detect others of a like kind adhering to the rocks, and it occurred to me that probably the waters being diluted

near the shore were not dense enough to deposit these crystals; and that if we could reach the island we might find them there, where, from the absence of fresh water springs, the saline solution of the lake would be more dense. This conjecture was fully verified. Mr. Semple, my secretary, succeeded in reaching the island in a very insecure boat, where he found the shores completely incrusted with beautiful clusters of these crystals, whose acute edges cut the naked feet. We secured an abundant supply of this rare and interesting mineral which, I believe, had not before been recognized in the United States. No other crystallized mineral was discovered.

The Gay-Lussite obviously has its origin from the reaction of the salts of soda and lime with which the waters are abundantly charged, and being very slightly soluble is readily deposited in these situations where the density of the water is maintained or increased by solar evaporation.[1] Hence it does not occur along the shores where the marginal springs of fresh water dilute the solution. The flow of these springs does not in summer fully replace the solar evaporation, as is evident from the water-line retiring slightly from its winter level.

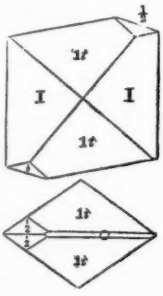
This interesting lake has no outlet. It has plainly been a point of volcanic activity in modern geologic times, its eruptions being confined to mud, ashes, pumice and lapilli. It is one of a considerable number of similar phenomena with which the Great Basin is dotted, and of which Mono lake, on the western margin of the Desert, is the most remarkable. The bottom of the ancient lake whose waters have left beautiful terrace-lines along the sides of the Humboldt and other mountain ranges, when these were either marginal shores or islands of this great mediterranean sea of fresh or brackish waters, is every where strewn with dead fresh-water modern shells, chiefly univalves.

ART. XXXIV.—*On crystals of Gay-Lussite, from Nevada Territory;* by JOHN M. BLAKE.

THE crystals of Gay-Lussite here described were obtained by Prof. B. Silliman in 1864, at Little Salt Lake, near Ragtown, Churchill Co., Nevada. The crystals differ strikingly from those measured and described by Phillips (Phil. Mag., April, 1827) in the proportional development of the planes as is shown by comparison with the figures given by Phillips, and by Descloizeaux (Ann. Ch. Phys., 3d series, vol. vii, p. 489).

[1] Gay-Lussite has been made artificially by J. Fritzsche, by mixing eight parts by measure of a saturated solution of carbonate of soda with one of a solution of chlorid of calcium of 1·130—1·150 specific gravity.—*J. f. pr. Ch.*, xciii, 339.

The planes observed by Phillips were *I*, *O*, $i\bar{i}$, $1\bar{i}$, $1\dot{i}$, and $\frac{1}{2}$; and the monoclinic axes, calculated from his measurements, are $a:b:c=1{\cdot}444:1{\cdot}489:1$; $C=78°\ 27'$. Of the above-mentioned planes, $i\bar{i}$ was not detected on the Nevada crystals. But there is a plane, not before mentioned, $i\ddot{i}$, which can be seen when the crystal is placed in the proper position to reflect the sunlight, and it then appears to be made up of numerous microscopic planes. The same was true of $1\dot{i}$. These two planes, giving no definite reflected image of the sun, were approximately measured by noting the points at which the light was reflected with the maximum intensity.



In my trials I found the cleavage parallel to planes *I* perfect; parallel to *O* less perfect, giving a reflected image with a strong light. Specimens in the Yale Cabinet from near Lake Maracaibo, South America, showed the same composite character of the planes; but the effloresced condition of the specimens prevented any exact comparison with them.

The following are the angles obtained: the faces are mostly too feebly polished to afford results nearer than a degree. The angles are given in the order in which they were obtained in the several zones.

Zone 1st: *I* on *I*, 69° 25′; *I*, 180° 20′; *I*, 247° 50′.

Zone 2d: $1\dot{i}$ on $1\bar{i}$, 69° 30′; *O*, 123° 20′; $1\bar{i}$, 177° 50′, 179° 40′; $1\dot{i}$, 249° 30′; *O*, 304°; $1\bar{i}$, 0°.

Zone 3d: *I* on $1\bar{i}$, 43° 20′; $\frac{1}{2}$, 71°, small; *I*, 180° 20′; $1\dot{i}$, 221° 20′; $\frac{1}{2}$, 249° 40′, small.

Zone 4th: *I* on $1\dot{i}$, 53° 10′; *I*, 179° 20′, 180° 10′; $1\dot{i}$, 231° 20′; *I*, 359°, 0°.

Zone 5th: *I* on $\frac{1}{2}$, 52° 50′; *O*, 96° 10′; *I*, 179° 10′; $\frac{1}{2}$, 231° 15; *O*, 274° 20′.

Zone 6th: *O* on $1\bar{i}$, 50°; $i\ddot{i}$, 101°; *O*, 178° 40′; $1\dot{i}$, 228° 30′? $i\ddot{i}$, 281°?

The following are Phillips's measurements, arranged in three zones, containing all of his observed planes:

Zone 1st: *I* on $i\bar{i}$, 34° 25′; *I*, 68° 50′; *I*, 180°.

Zone 2d: $1\dot{i}$ on $i\bar{i}$, 35° 15′; $1\dot{i}$, 70° 30′; *O*, 125° 10′; $1\dot{i}$, 180°.

Zone 3d: *I* on $1\bar{i}$, 42° 15′; $\frac{1}{2}$, 69° 55′; $1\bar{i}$, 110° 20′; *I*, 180°.

New Haven, Ct., Jan. 1866.

ART. XXXV.—*On the Structure and Habits of Anthophysa Mülleri* Bory, *one of the sedentary monadiform Protozoa;* by H. JAMES-CLARK, A.B., B.S.

DURING the last five years, and more especially within the latter eighteen months I have been engaged largely upon an investigation of the relations of the monadiform animalcules to the zoöspores of the true *Algæ;* and of all the numerous instances of the former that I have more or less thoroughly studied I have never met with one which could be said to bear but a very moderate resemblance to the latter. I refer to the true *Algæ*. I scarcely need add that I mean by this to except those doubtful forms which seem to be related to *Volvox* and *Gonium*, such as *Pandorina*, *Protococcus*, *Stephanosphæra*, *Chlamidococcus*, &c.

Those who have become accustomed to these creatures, and have learned to look upon them, through long years of patient study, as old and familiar friends, know well the value of using the best lenses that the opticians of the present day can afford; and never doubt for a moment the utter worthlessness of an opinion which is founded upon a few fitful glances through a so-called ordinary working microscope. There is no other group of animals which so essentially seems to need the prolonged devotion of a specialist as the *Protozoa;* and above all the lower members of that grand division. To write a monograph upon any single one of these flagellate forms may seem like devoting a volume to the structure and phases of a dot in a sunbeam, but no good microscopist need be told that the optical instruments of the present day are no less efficient than was the scalpel in the hand of Cuvier when he displayed to the world the organization of the larger and more elevated animals which he found on the southern shores of France.

Moreover it is particularly desirable that elaborate investigations should be made, and unstinted minutiæ set forth in illustrations and descriptions, because there are yet among zoölogists those who suppose that there is so little in the organization of Protozoa that no tangible characters can be found by which they may be typified, or assimilated in a group by themselves.

The taxonomic relations of the organs of the *Infusoria Flagellata* have received so little attention from investigators that there is no small difficulty, with our present knowledge of them, in tracing the typical plan which is so eminently exemplified among the *Ciliata*. I hope I shall be pardoned, therefore, if I attempt to give a strict topographical view of the positions of the various organs of one among the most lowly of the whole group of animalcules.

A considerable portion of the second volume of the great work of Messrs. Claparède and Lachman, "Etudes sur les Infusoires," &c., is occupied by a discussion of the animality of certain doubtful forms of Monad-like infusoria. The tests which these authors offer as determinatives of the zoölogical relations of the forms in question are the possession by them of a contractile vesicle and the introception of food. By means of either the one or the other of these criteria they succeed in satisfying themselves that the *Volvocina*, *Astasiæ* (*Euglenæ* included), and the *Dinobrya* are true animals; but in regard to other forms they are unable to decide. Among those which are left in the latter category, there is a singular infusorian which, as is usually supposed, was originally named *Epistylis vegetans* by Ehrenberg, and *Anthophysa Mülleri* by Bory de St. Vincent. Dujardin gives a scarcely recognizable figure of it in the atlas of his work on Infusoria; but very properly places it among the monadiform animalcules. This is done, however, upon its general resemblance to the latter—alike undetermined at that date as to their animal nature—and not because he had by direct observation decided it to be a genuine animal. The figures of Cohn (Mikroskopische Algen und Pilze. Nov. Acta Acad. Cæs, Leop, 1854. Taf. xv, fig. 1–8) are not much better than those of Dujardin.

Habitat and general appearance.—I have been so fortunate as to determine the animality of *Anthophysa* by both of the tests above mentioned; and there rests not the least doubt in my mind that this infusorian is as truly a member of the zoölogical kingdom as any of the well known Protozoa. I would state, for the information of those who are not acquainted with the habits of this animalcule, that it is quite common among the fresh water weeds. It may be most advantageously studied when it is attached to *Myriophyllum* or *Ceratophyllum;* a small piece of the tip of the filiform leaf, of either, which seems to be covered by an irregular, floccose deposit, usually affording abundant specimens.

Under a low magnifying power this floccose matter appears to consist of clusters of very jagged, irregularly branching and contorted, semitransparent, intertwined stems and projecting, tapering and flexible twigs. Each of the tips of the latter sustains a single, more or less globose mass of spindle-shaped bodies, which radiate from a common center of attachment; and are kept in a constant agitation by the spasmodic jerks of a long, stout, usually rigid, arcuate filament, with which the free end of each one is endowed. The whole bristling mass revolves alternately from right to left and from left to right; whirling upon its slender pivot with such a degree of freedom that one might almost suspect that it merely rested upon it, and had no truer adhesion to it than the juggler's top to the end of the

baton upon which it spins. The largest of these twirling groups contains as many as fifty fusiform bodies, but most frequently not more than half that number are grouped together; and from this they vary in decreasing quantities down to only one or two upon each filamentous twig. In the last instances the bodies are comparatively quiet, scarcely moving out of focus at each spasmodic twitch of the arcuate filament. On this account, and because they offer an unobstructed view, the latter are by far the most available as objects for the investigation of their internal organization.

The relationship of the individual monads to the whole colony must, however, be studied where they are more numerously congregated; since, as will be shown presently, each monad sustains a definite relation to every other one, and to the twig to which it is attached. The larger colonies are frequently to be found swimming freely, with a rolling motion similar to that with which *Volvox* progresses. As a natural concomitant to this fact, twigs are to be met with, here and there, which do not bear anything at their tips. The colonies seem to break away very easily; and on this account the specimens should not be lifted out of the water when transferring them to the watch-glass or whatever sort of observing-trough is used.

Form, &c.—The adult monads have a truncate fusiform shape, and are slightly, but quite appreciably flattened on two opposite sides; so that in an end view they appear to be broadly oval transversely. The attached end tapers gradually to a point; and on this account it is difficult to determine where the body ends and the twig begins. All of the members of a group radiate from a common point of attachment, to which they adhere by their tapering filamentous ends. The free end is truncate, but one corner of it,—as if in continuation of the line along which the opposite flattened sides meet,—projects in the form of a rather blunt triangular beak. At the inner edge of the base of this beak lies the mouth, to which the former—as frequent observation has proved—acts as a lip or prehensile organ when food is taken into the body. The prevailing tint is a more or less uniform light gamboge, without the least trace of an eye-spot of any color.

A most singular uniformity prevails in the arrangement of the several members of a group. Each monad is attached to its mooring in such a position that its flattened sides lie parallelwise with those of its nearest neighbor; and the beak projects from that corner of the head which is most distant from the twig. To give a full idea of the peculiarity of this arrangement it must be stated here that the rigid, arcuate, spasmodically twitching filament mentioned above is attached close to the mouth, and invariably curves away from the beak, and consequently

always toward the pedicel of the colony. One is forcibly reminded by this of the systematic relation of some of the flowers of *Labiatæ*, with their stamens projecting far beyond the upper lip of the corolla. The globose heads of the *Menthæ* are particularly good examples for illustrating this similitude.

Prehensile organs.—The only motile organs which this animalcule possesses are preëminently prehensile in character; and their apparent appropriation for the office of propulsion, when a colony breaks loose from its attachment, I can scarcely doubt is an accidental one, inasmuch as the arcuate cilium continues its spasmodic twitching without any apparent deviation from its usual mode of action.

There are two cilia, of very unequal size, attached to the truncate end of the body. The larger one of these has already been mentioned casually, as a *rigid, arcuate filament.* It does not taper, but has a uniform thickness from base to tip, and is about half again as long as the body. It arises near the base of the triangular beak, but appears to be separated from the latter by the intervening mouth. When quiet it appears like a bristle, and projects in a line with the longer axis of the body; at the base bending slightly toward the beak, and then sweeping off in a moderate but distinct curve in the opposite direction, so that on the whole it presents a long drawn out sigmoid flexure. The plane of this curve lies in strict parallelism with the plane of the greater diameter of the body; in fact it may be said to be a direct continuation of it. It does not appear to have the character of a *flagellum*, except when assisting the smaller cilium to convey the food to the mouth; and then it lays aside its rigid deportment and assumes all the flexibility and wavy vibration of the prehensile organ of an *Astasia.*

The smaller cilium is an excessively faint body, and almost defies the detective powers of the highest objectives. This is partly due to its almost incessant activity; for when it is quiet, or nearly so—which happens when food is passing into the mouth—it becomes comparatively quite conspicuous under a one-eighth of an inch objective. It is scarcely as long as the greater diameter of the truncate end of the body. It arises close to the base of the larger cilium, but whether on the right or left, or nearer or more distant from the mouth than the latter cannot be said positively. Most frequently it was observed to be flexed in the same direction as its companion; and occasionally it seemed to be quite evident that it was attached nearer to the mouth than the latter. It is highly flexible and vibrates with great rapidity in what appears to be a gyratory manner.

The mouth.—This organ is never visible except when food is passing through it. It then may be seen that it lies close to the beak, which acts as a sort of lip by curving over the introcepted

particles as they pass into the body. The mouth is highly distensible; at times allowing particles as wide as two-thirds the greater diameter of the body to pass in without any apparent extra effort. It seems undeniable that it possesses discriminative powers in regard to the quality of its food. This one may readily judge of for himself, by seeing the unerring precision with which the particles of floating matter are thrown, by the spasmodic incurvature of the larger flagellum, against the mouth, where, if they are not swallowed, they are detained but for an instant by the smaller cilium, quickly adjudged to be worthless, and then thrown off with a twirl of the organ which held them in temporary abeyance. If, however, the captured morsel proves to be agreeable, the larger cilium assists the operations of the smaller one, and the lip, by abruptly bending itself at its point of attachment and laying its basal part across the food, and pressing it into the mouth, while the terminal portion is kept in a constant wavy vibration, and curved toward the posterior end of the body. This is usually done in three or four seconds and then the cilia return to their usual positions, while the introcepted edible passes toward the center of the body, and is there immediately enclosed in a digestive vacuole. For a while the food dances about in this vacuole with a very lively motion, but finally it subsides into quietude.

The contractile vesicle.—There is a two-fold difficulty in discovering the presence of this organ. In the first place it is comparatively quite small, and secondly it pulsates so slowly that it is very rarely possible to see it contract twice in succession between any two of the abrupt, lateral deviations of the body, which the spasmodic twitchings of the arcuate flagellum produces. On this account it has not been possible to determine the precise rate of its systole and diastole. It seems to contract from three to four times a minute. It lies near the surface, about half way between the two ends of the body, and nearly midway betwixt the two extremes of its greater diameter. At the completion of its *diastole* it has a circular outline, and appears like a clear colorless vesicle in the midst of the yellowish tissue of the body. Upon contraction it disappears and leaves no trace of its presence. The *systole* progresses slowly, as in *Anisonema* (*A. sulcata* Duj.? and *A. nov. sp.*), *Cyclidium* (*C. nov. sp.*), and *Phacus pleuronectes* Duj.; and in this respect contrasts strongly with the same process in *Heteromita fusiformis* Jas.-Clk., *Astasia tricophora* Clap., and *Cryptomonas* (*C. nov. sp.*), in which the last half of the systole is very abrupt and marked.

The stem.—In addition to what has already been said of the general appearance of this part of the organism it may be added that the older and basal portions of the branches are flat, and have a distinct longitudinal, irregular striation; to all appear-

ances made up of the older, laterally agglutinated twigs. The youngest, terminal portions of the branches which, under the name of twigs, have been described in this paper as the immediate supporters of the colonies of monads, are evidently tubular. They appear to be as flexible as a spider's thread, and are usually quite irregular in outline, and in the calibre of the canal which permeates them. The wall of these tubular twigs is quite thick, and is alike rough on the exterior and interior faces. The substance within the tubes appears homogeneous, but whether it is solid or fluid could not be determined. The oldest part of the stems is of a reddish brown color, but as they taper off into branchlets they gradually assume a gamboge color, and finally terminate in scarcely colored twigs.

Reproduction by fissigemmation is the only method of propagating individuals which I have observed. As a preliminary to this process the monad gradually loses its fusiform shape, and assumes at first an oval contour and finally becomes globular. During this transition both of the prehensile cilia become much more conspicuous than usual, and the body develops a closely fitting hyaline envelope about it; thus passing into a sort of encysted state. The contractile vesicle, however, does not seem to cease its pulsations during this period, and moreover it becomes quite conspicuous. This arises mostly from the fact that the body is in a nearly quiet state, and allows the observer to obtain a prolonged and undisturbed view of it. Unfortunately the rate of the pulsations of this organ was not ascertained when the following observations were made, because the whole time was occupied in watching and drawing the various and rapidly changing phases of self-division.

After the body assumes a globular shape, as above mentioned, both the larger and smaller cilium seems to be undergoing a change, and becomes indistinct in outline.[1] Presently two larger

[1] In a new fresh-water genus (see note 2) of sedentary, monadiform Protozoa—possessing two contractile vesicles, and only the *sigmoid flagellum*, the latter arising within a deep bell-like flange, or projecting rim which embraces the anterior end of the body—this arcuate filament disappears altogether by a sort of withering down from tip to base—reminding one of the shrivelling of the end of a cotton thread in the flame of a lamp—preliminary to the commencement of the longitudinal fissigemmation of the body and its bell-like flange; and then the new flagellum of each resultant of self-division grows out in about twenty minutes.

[2] *Codosiga:* κωδων a bell, σιγαω to be silent. *C. pulcherrima*, n. sp. Body obliquely obovate, and tapering at its posterior end into a slender pedicel; truncate and abrubtly constricted in front where the base of the bell meets the body. Sigmoid, arcuate flagellum as long as the body and bell. The two contractile vesicles in the posterior third of the body; superficial, large, and quite conspicuous; each contracting, alternately with the other, once in about half a minute. Bodies attached, in groups of from two to eight, by their pedicels to the tip of a slender stem; erect or divergent, but not pendent. Mouth at the base of the flagellum, i. e., terminal. Anus near the mouth. No eye-spot. Bell slightly flaring; half again deeper than broad; fully as deep as the length of the body; highly contractile. Color of the body—excepting the hyaline bell—pedicels and stem deep yellow. Common on fresh-water weeds about Cambridge.

flagella burst upon the view, apparently by the longitudinal splitting of the previously single one of the same kind, and rapidly separate from each other by the broadening of the body, and leave between them the smaller cilium. The latter at this time appears much thicker than usual, and seems to be composed of two closely approximated, parallel threads. By this time the contractile vesicle has also divided into two, which lie closely side by side.

At this moment the time noted in one series of observations was 2.30 P.M. By 2.35 P.M. the larger flagella had separated still farther, and the smaller cilium had split into two very conspicuous filaments; as yet, however, attached to a common point of the body. From this time forth to the completion of the process of fissigemmation all of the cilia kept up a slow vibration, in which they undulated from base to tip with a sort of snake-like motion. By 2.45 P.M. the body had become quite appreciably broader than long; the contractile vesicles were widely separated, and the smaller cilia had left between them a considerable space, and each one had approximated quite near to the base of a larger flagellum. At 2.50 P.M. the body had become nearly twice as broad as long, and the space between the two pairs of cilia was nearly twice as great as in the last phase, and considerably depressed in the middle, so that the body had a broadly cordate outline. By 2.52 P.M. the posterior end of the body—at a point a little to one side of the spot where it was attached to the pedicel—was also slightly indented, so that in outline it presented a guitar-shaped figure, each rounded half of which bore a pair of unequal cilia, and contained a contractile vesicle. In one minute more the contraction had increased to such an extent that the body was divided about half way through. By 2.54 P.M. the animal had a dumb-bell shape, and the pedicel was attached to one of the segments near the point of constriction. Still the process went on very rapidly, and by 2.55 P.M. the new bodies were widely separated, but still attached to each other by a mere thread. At 3 P.M. the body which was attached to the pedicel was left alone, and its companion swam away to seek a new attachment, and build up its stem.

To the last moment the hyaline envelope remained about the segments, and in fact so long afterwards that time and circumstances did not allow me to ascertain its final disposition. I would remark, however, that when the ovate bodies of the half grown monads are contracted temporarily into a globular shape, they appear identical—excepting that they lack the hyaline envelope—with these recently fissated forms. In all probability, therefore, the latter lose their envelope and assume the shape of the former.

As to the development of the stem I think it quite certain that it grows out from the posterior end of the body. The best proof of this is that I have frequently found a monad—especially in the condition of the one which I described above as breaking loose from its companion—nearly sessile upon a clean spot, and attached by a very short, faint, film-like thread. From this size upward I had no difficulty in finding abundant examples as gradually increasing in diameter as they did in length; thus furnishing a pretty strong evidence that the stem grows under the influence of its own innate powers, and is not therefore a deposit emanating from the body of the monad, except, perhaps, as far as it may be nourished by a fluid circulating within its hollow core.

Cambridge, Mass., May 21, 1866.

ART. XXXVI.—*Address of Prof. DeCandolle to the recent Botanical Congress in London.*[1]

IN order to derive the full advantage from a meeting of so many lovers of science, horticulturists and botanists, brought together from all parts of Europe, it is necessary that the common object for which they have met should be perfectly understood.

It devolves on me, who am called upon to preside (an honor of which I feel myself unworthy), to point out the bond which unites us, and of which perhaps you have at present but a vague, and, so to speak, an intuitive perception.

In my opinion, we are not here merely as amateurs to satisfy our curiosity. The proof of which is, we are here assembled to listen to discussions, instead of wandering about the fairy-like garden of the Exhibition. Evidently we seek something more than a mere flower show, and that something is, in my opinion, instruction. It is not sufficient for horticulturists merely to see —they must also study and reflect; neither is it sufficient for botanists to observe details minutely; they must also see the plants on a large scale and in grouped masses. The connection of practice with theory, and of art with science, is acknowledged to be indispensable; and in accordance with this prevalent opinion we here affirm, by our presence in this room, the necessary union of botany and horticulture. The aim of my brief observations will be to call to mind how they aid each other, and to

[1] The first meeting of the Botanical Congress was held in the Raphael Room of the South Kensington Museum on Wednesday, May 23, at 11 A. M., Prof. DeCandolle in the chair. A very large meeting, including almost all the British and foreign botanists and horticulturists present in London were assembled to hear the President's address.

show how much more they might do so. If I am not mistaken, it will follow from the facts to which I shall allude, that our united efforts, scientific or practical, modest though they appear, contribute to increase the well-being of man, in all conditions and in all countries.

1. *The advantages of horticulture to botany.*—Let us first mention the services that horticulture renders, or may render, to botany. Without being myself a horticulturist, I affirm or recognize them willingly, the advancement of science rendering it necessary to have recourse to all its collateral branches.

We no longer live in those times of illusion, when botanists merely occupied themselves with European plants, or with a few from the East, and, from a spirit of caution rather than from ignorance, pictured to themselves all distant countries as possessing much of the same general vegetation, with a few uncommon or exceptional species. A century of discovery has made known the extreme variety in the floras, the restricted limits of many species, and the complicated entanglement of their geographical distribution. To see all the different forms of vegetation of the world, would be to realize in a degree the history of the Wandering Jew; besides, with this constant travelling, where would be the opportunities for that reflection or study which create true science?

The traveller is too much exhausted in warm countries, too distracted in those temperate regions favorable to active life, and his faculties are too much benumbed in the colder regions, to enable him to devote himself to minute researches with the lens or the microscope, or even to sketch or properly describe that which he has gathered. He sees, in passing, a crowd of things, but he can scarcely ever stop to enter into details, especially of those that present themselves in rapid succession. Rarely can he see the fruit and flower of a species at the same time, and it is quite impossible for him to study their complete development during the whole year. The notes taken by the most intelligent naturalist are so affected by these fatal circumstances, that it is seldom they add anything to that which a dried specimen can teach the sedentary botanist.

It is horticulture, then, which brings before us a multitude of exotic plants in a condition best adapted for study. Thanks to the variety of species it accumulates and successfully cultivates, the botanist can investigate the most difficult questions, and pursue his researches in families whose genera are not indigenous in Europe. In the herbarium, more minute observations can be made than is generally supposed; nevertheless, for certain researches, it is absolutely necessary to have the living plant, particularly for those relating to the relative disposition, the origin and development of the several organs, as well as for

studying the curious phenomena of fertilization, the movements and direction of the stem, leaves, and parts of the flowers. Horticulture has done much to advance the progress of physiological botany, but it still has much to do. The most remarkable experiments of physiologists—viz., those of Hales, Duhamel, Knight—have been made in gardens. Also the long series of experiments of the younger Gaertner, and, more recently, of M. Naudin, on hybridization, which relate to the cardinal subject of the species. As much may be said of the numerous trials which are made, in horticultural establishments, to obtain new races or varieties. These have a great scientific importance, and it is undoubtedly the horticulturists who are the teachers of botanists on these subjects.

It appears to me, however, gardens can be made still more useful in carrying out physiological researches. For instance, there is much yet to be learned on the mode of action of heat, light, and electricity upon vegetation. I pointed out many of these deficiencies in 1855, in my "Géographie Botanique Raisonnée."[2] Ten years later Mr. Julius Sachs, in his recently published and valuable work on physiological botany,[3] remarks much the same deficiencies, notwithstanding that some progress has been made in these matters. The evil consists in this, that when it is desired to observe the action of temperature, either fixed or varied, mean or extreme, or the effect of light, it is exceedingly difficult, and sometimes impossible, when observations are made in the usual manner, to eliminate the effects of the constant variations of heat and light. In the laboratory it is possible to operate under more exactly defined conditions, but they are rarely sufficiently persistent; and the observer is led into error by growing plants in too contracted a space, either in tubes or bell-glasses. This last objection is apparent when it is wished to ascertain the influence of the gases diffused in the atmosphere around plants, or that of the plants themselves upon the atmosphere.

Place plants under a receiver, and they are no longer in a natural condition; leave them in the open air, and the winds and currents, produced at each moment of the day by the temperature, disperse the gaseous bodies in the atmosphere. Every one is aware of the numerous discussions concerning the more or less pernicious influence of the gases given off from certain manufactories. The ruin now of a manufacturer, now of a horticulturist, may result from the declaration of an expert; hence it is incumbent on scientific men not to pronounce on these delicate questions without substantial proof.

[2] Pages 46, 49, 57, and 1346.

[3] Handbuch der Experimental-physiologie de Pflanzen, 1 vol. in 8vo. Leipzig, 1865.

With a view to these researches, of which I merely point out the general nature, but which are immensely varied in details, I lately put this question[4]—"Could not experimental greenhouses be built, in which the temperature might be regulated for a prolonged time, and be either fixed, constant, or variable, according to the wish of the observer?" My question passed unnoticed in a voluminous work where, in truth, it was but an accessory. I renew it now in the presence of an assembly admirably qualified to solve it. I should like, were it possible, to have a greenhouse placed in some large horticultural establishment or botanic garden, under the direction of some ingenious and accurate physiologist, and adapted to experiments on vegetable physiology; and this is, within a little, my idea of such a construction:—

The building should be sheltered from all external variations of temperature; to effect which I imagine it should be in a great measure below the level of the ground. I would have it built of thick brickwork, in the form of a vault. The upper convexity, which would rise above the ground, should have two openings—one exposed to the south, the other to the north—in order to receive the direct rays of the sun, or diffused light. These apertures should each be closed by two very transparent glass windows, hermetically fixed. Besides which, there should be, on the outside, means of excluding the light, in order to obtain complete darkness, and to diminish the influence of the variations of temperature when light was not required. By sinking it in the ground, by the thickness of its walls, and by the covering of its exterior surfaces with straw, mats, &c., the same fixed degree of temperature could be obtained as in a cellar. The vaulted building should have an underground communication with a chamber containing the heating and the electrical apparatus. The entrance into the experimental hothouse should be through a passage closed by a series of successive doors. The temperature should be regulated by metallic conductors, heated or cooled at a distance. Engineers have already devised means by which the temperature of a room, acting on a valve, regulates the entry or exit of a certain amount of air, so that the heat regulates itself.[5] Use could be made of such an apparatus when necessary.

Obviously, with a hothouse thus constructed, the growth of plants could be followed from their germination to the ripening of their seeds, under the influence of a temperature and an amount of light perfectly definite in intensity. It could then be ascertained how heat acts during the successive phases from

[4] Géographie Botanique, 1855, pp. 49 and 1346.

[5] See the electrical apparatus of M. Carbonnier, exhibited at Chiswick in 1837, figured in the "Flore des Serres et Jardins," vol. xii, Miscell. p. 184.

sowing to germination, from germination to flowering, and from this on to the ripening of the seed. For different species various curves could be constructed to express the action of heat on each function, and of which there are already some in illustration of the most simple phenomena, such as germination,[6] the growth of stems, and the course of the sap in the interior of certain cells.[7] We should be able to fix a great number of those minima and maxima of temperature which limit physiological phenomena. Indeed, a question more complicated might be investigated, toward the solution of which science has already made some advances, namely, that of the action of variable temperatures; and it might be seen if, as appears to be the case, these temperatures are sometimes beneficial, at other times injurious, according to the species, the function investigated, and the range of temperature. The action of light on vegetation has given rise to the most ingenious experiments. Unfortunately, these experiments have sometimes ended in contradictory and uncertain results. The best ascertained facts are, the importance of sunlight for green coloring, the decomposition of carbonic acid gas by the foliage, and certain phenomena relating to the direction or position of stems and leaves. There remains much yet to learn upon the effect of diffused light, the combination of time and light, and the relative importance of light and heat. Does a prolonged light of several days or weeks, such as occurs in the polar regions, produce in exhalation of oxygen, and in the fixing of green matter, as much effect as the light distributed from 12 to 12 hours, as at the equator? No one knows. In this case, as for temperature, curves should be constructed, showing the increasing or diminishing action of light on the performance of each function; and as the electric light resembles that of the sun, we could in our experimental hothouse submit vegetation to a continued light.[8]

A building such as I propose would allow of light being passed through colored glasses or colored solutions, and so prove

[6] Germination under different degrees of constant heat, by Alph. de Candolle, in the "Bibliothèque Universelle de Genève" (Archives des Sciences), Nov. 1865.

[7] If the curves had not been constructed, the data for their construction are, at least, dispersed throughout our books. I will cite, for instance, the growth of a scape of Dasylirion, as observed by M. Ed. Morren (Belgique Hortic., 1865, p. 322). The figures there given are not favorable to the accepted notion, that the growth of tissues is more active by night than by day.

[8] The apparatus which produces the most persistent and vivid light is the magneto-electric machine, based on the development of induction by magnetism, as discovered by the illustrious Faraday. The galvanic pile is replaced by a steam-engine of low power, which sets in motion a wheel furnished with magnets (Bibl. Univ. de Genève, Archives Scientif., 1861, vol. x, p. 160). The working of this machine is inexpensive, but, unfortunately, the magnets are very costly. This system has already been applied to two lighthouses—that at the South Foreland, and to that of the "Société l'Alliance," at Havre—in consequence of the experiments of MM. E. Becquerel and Tresca.

the effect of the different visible or invisible rays which enter into the composition of sunlight. For the sake of exactness nothing is superior to the decomposition of the luminous rays by a prism, and the fixing the rays by means of a heliostat. Nevertheless, a judicious selection of coloring matters, and a logical method of performing our experiments, will lead to good results. I will give as proof, that the recent most careful experiments concerning the action of various rays upon the production of oxygen by leaves and upon the production of the green coloring matter, have only confirmed the discoveries made in 1836, without either prism or heliostat, by Professor Daubeny,[9] from which it appears that the most luminous rays have the most power, next to them the hottest rays, and lastly those called chemical.

Dr. Gardner in 1843, Mr. Draper immediately after, and Dr. C. M. Guillemin in 1857,[10] corroborated by means of the prism and the heliostat the discovery of Dr. Daubeny, which negatived the opinions prevalent since the time of Senebier and Tessier, and which were the result of erroneous[11] experiments. It was difficult to believe that the most refrangible rays—violet for instance, which acts the most on metallic bodies—as in photometrical operations, should be precisely those which have least effect in decomposing the carbonic acid gas in plants, and have the least effect over the green matter in leaves. Notwithstanding the confirmation of all the experiments made by Dr. Daubeny, when repeated by numerous physicists and by more accurate methods, the old opinions, appearing more probable, still influenced many minds,[12] till Mr. Julius Sachs, in a series of very important experiments, again affirmed the truth.[13] It is really the yellow and orange rays that have the most power, and the blue and violet rays the least, in the phenomena of vegetable

[9] Daubeny, Philos. Trans., 1836, part 1.

[10] Dr. Gardner, Edinb. Phil. Mag., 1844, extract in French in La Biblioth. Univ. de Genève, February, 1844; Draper, Edinb. Phil. Mag., September, 1844, extract ib., 1844, vol. liv; Guillemin (C. M.), Ann. Sci. Nat., 1857, ser. 4, vol. vii, p. 154.

[11] Senebier, Mém. Phys. et Chim., ii, p. 69; Tessier, Mém. Acad. Sci., 1783; Gilby, Ann. de Chimie, 1821, xvii; Succow, Commentatio de lucis effectibus chemicis, in 4to, Jena 1828, p. 61; Zantedeschi, cited by Dutrochet, Compt. Rend. Acad. Sci., 1844, sem. 1, p. 853.

[12] As a proof of the persistence of the old opinion, I will quote a phrase of Professor Tyndall's, in his most clear and interesting treatise "On Radiation," (London, 1865,) p. 6:—"In consequence of their chemical energy, these ultra-violet rays are of the utmost importance to the organic world." I do not know whether the author had in view an influence of the chemical rays over the animal kingdom; but, according to certain passages of Mr. Sachs, I doubt if they have more power over animals than they have over plants; besides, Professor Tyndall did not concern himself with these questions; he was content to explain admirably the physical nature of the various rays.

[13] The researches of Mr. Sachs first appeared in the Botanische Zeitung: they are collected and condensed in the remarkable volume called Handbuch der Physiologischen Botanik, vol. iv, Leipsig, 1865, pp. 1 to 46.

chemistry; contrary to that which occurs in mineral chemistry, at least in the case of chlorid of silver. The least refrangible rays, such as orange and yellow, have also the twofold and contrary property, such as pertains also to white light, and which produces the green coloring matter of leaves or bleaches them, according to its intensity. It is these, also, which change the coloring matter of flowers when it has been dissolved in water or alcohol.[14] Those rays called chemical, such as violet, and the invisible rays beyond violet, according to recent experiments, confirmatory of those of ancient authors—those of Sebastian Poggioli, in 1817,[15] and of C. M. Guillemin—have but one single well-ascertained effect, that of favoring the bending of the stem toward the quarter from which they come more decidedly than do other rays; yet that is an effect perhaps more negative than positive, if the flexure proceeds, as many still believe, from what is going on on the side least exposed to the light.[16]

The effect upon vegetation of the non-visible calorific rays at the other extremity of the spectrum have been but little studied. According to the experiments we have on this subject, they would appear to have but little power over any of the functions; but it would be worth while to investigate further the calorific regions of the spectrum by employing Dr. Tyndall's process, that is, by means of iodine dissolved in bisulphid of carbon, which permits no trace of visible light to pass.

How interesting it would be to make all these laboratory experiments on a large scale! Instead of looking into small cases, or into a small apparatus held in the hand, and in which the plants cannot be well seen, the observer would himself be inside the apparatus, and could arrange the plants as desired. He might observe several species at the same time, plants of all habits, climbing plants, sensitive plants, those with colored foliage, as well as ordinary plants. The experiment might be prolonged as long as desirable, and, probably, unlooked-for results would occur as to the form or color of the organs, particularly of the leaves.

Permit me to recall on this subject an experiment made in 1853 by Professor von Martius.[17] It will interest horticulturists now that plants with colored foliage become more and more fashionable. Prof. von Martius placed some plants of *Amaranthus tricolor* for two months under glasses of various colors. Under

[14] Sir John Herschell, Edinb. Phil. Journ., January, 1843.

[15] S. Poggioli, Opuscoli Scientifici, quoted by Dutrochet, Compt. Rend. Acad. Sci., 1844, sem. 1, p. 850.

[16] The rather confused and questionable explanations, founded on the notions of Dutrochet, of the existence of a deoxydizing power on the brightest side, clash with the fact that the blue, indigo, and violet rays, the least powerful for deoxydizing tissues, are the most powerful in causing them to bend.

[17] "Gelehrte Anzeige," München, Dec. 5, 1853.

the yellow glass the varied tints of the leaves were all preserved. The red glass rather impeded the development of the leaves, and produced at the base of the limb yellow instead of green; in the middle of the upper surface, yellow instead of reddish-brown, and below, a red spot instead of purplish-red. With the blue glasses, which allowed some green and yellow to pass, that which was red or yellow in the leaf had spread, so that there only remained a green border or edge. Under the nearly pure violet glasses the foliage became almost uniformly green. Thus, by means of colored glasses, provided they are not yellow, horticulturists may hope to obtain at least temporary effects as to the coloring of variegated foliage.

The action of electricity on foliage is so doubtful, so difficult to experiment upon, that I dare hardly mention it; but it can easily be understood how a building constructed as proposed might facilitate experiments on this subject. Respecting the action of plants on the surrounding air, and the influence of a certain composition of the atmosphere upon vegetation, there would be by these means a large field open for experiments. Nothing would be easier than to create in the experimental hothouse an atmosphere charged with noxious gas, and to ascertain the exact degree of its action by day and by night. An atmosphere of carbonic acid gas might also be created, such as is supposed to have existed in the coal period. Then it might be seen to what extent our present vegetation would take an excess of carbon from the air, and if its general existence was inconvenienced by it. Then it might be ascertained what tribes of plants could bear this condition, and what other families could not have existed, supposing that the air had formerly had a very strong proportion of carbonic acid gas.

Until horticulture can supply physiology with such convenient means of experiment, it, in the meantime, advances descriptive botany by the valuable publications it issues. The greater part of the old works with plates, such as "Hortus Eystettensis," "Hortus Elthamensis," &c.; also those of Ventenat, Cels, Redouté, &c.; the Salictum and Pinetum of the Duke of Bedford; and more recently the "Rhododendrons of the Himalaya," by Dr. Hooker; the works of Bateman, Pescatore, Reichenbach fils, on Orchids; and many others I could name, would never have existed, had there not been rich amateurs either to edit or buy them.

It is horticulture that has given us the longest series of illustrated journals that have ever been published; and here I must do justice especially to the English horticulturists. No doubt the science of our time requires a larger amount of analytical details than is contained in the plates of the "Botanical Magazine," "Botanical Register," "Andrews' Repository," "Loddiges'

Botanical Cabinet," "Sweet's British Flower Garden," "Paxton's Magazine and Flower Garden," and other English journals; but what a number of forms are thus fixed by the engravings in these books, and what a fund of valuable documents for consultation they afford. One must admire the "Botanical Magazine," commenced in 1793, continued from month to month with an exemplary regularity, and which is now at its 5580th plate. Not only has it always represented rare and new species, but it has ever been conducted on a simple and uniform plan, which renders it convenient to consult.

The series of plates is unique from the very beginning. Each plate has its number, and each article of letter-press refers only to one plate, by which means the quotations from the work are rendered brief and clear. Many editors have not understood the advantage of this simple arrangement. They have varied their titles, their series, their pagings; they have affixed to their plates numbers, then letters, then nothing at all; the end of which is (and this ought to serve as a warning for the future) that the more they have altered and complicated the form of their journals, the shorter time they have lasted.

How is it that these purely bibliographical details cause in us such sad recollections? Of the men just mentioned, who have rendered such eminent service to botany and horticulture, England has lost three during the year 1865—Sir Joseph Paxton, Dr. Lindley, and Sir William Jackson Hooker.[18] I should certainly fail in what is expected of me if I did not express, in the name of the foreigners attending this meeting, our deep regret at such serious losses. We know them all by their writings, and many among us have known personally the distinguished men I have mentioned. Their names follow us at each step in this the scene of their labors. If we admire the boldness of construction of the iron domes that characterize modern buildings, we think of the Crystal Palace, of Chatsworth, and of the humble gardener who became a great architect. If we visit the beautiful establishment at Kew, we see everywhere around us proofs of the indefatigable activity of Sir William Hooker. Lastly, if we ask the origin of the garden of the Royal Horticultural Society at Kensington, we are told it is only a development of that at Chiswick, where Lindley stood preëminent by his knowledge and his energy; and of that Society where botanists of my age found in their youth such valuable encouragement in their studies.

The names of Sir William Hooker and of Dr. Lindley, thanks

[18] Since these lines were in the printer's hands, British science has sustained a severe loss in the death of the truly amiable and learned Professor W. H. Harvey, of Dublin, so well known by his works on *Algæ*, and on the botany of South Africa. I cannot refrain from expressing our sense of this great bereavement.

to their special works, will ever remain distinguished in science. These two botanists have, moreover, been directors of horticultural journals, and of great horticultural establishments, and since their influence has been so fully acknowledged by practical men, I shall have little trouble in showing that science is as useful to horticulturists as horticulture is to botanists—and this will form the second part of my discourse.

2. *The advantage of botany to horticulture.*—The principles of vegetable physiology are what horticulturists and agriculturists usually study in books on botany. They do not always find direct answers to their questions; but they can draw from them certain rules, certain ways of experimentalizing and reasoning, which saves them from falling into many errors. Should some ridiculous idea be promulgated by some ignoramus or charlatan, it is by an appeal to the general rules of physiology that a practical man may at once reject them, or, at least, hold them in distrust. On the contrary, innovations, if in harmony with the principles, may be, and I will even say ought to be, readily accepted.

Do not let us put too much faith in the lucky results of experiments made absolutely by chance. It is with some of these experiments as with dreams and presentiments—if they come true once in a thousand times they are talked about, otherwise they are passed over and forgotten. Besides, it must be said, men nearly always are guided by theories; but the theories of the ignorant are often absurd and without foundation, while those of educated men are based on probabilities, or on an accumulation of facts.

Conjointly with physiology, botanical geography shows the distribution of plants all over the globe, their struggle with the elements, their migrations, and already raises a portion of the veil which covers the obscurity of their origin. All this ought to offer a real interest to horticulturists. We are beginning to have the power of expressing in figures the effect of each climate upon vegetation; consequently, the possibility of a given species enduring the mean or extreme climatal conditions of that country to which it is desired to introduce it. Already we can show, in the clearest manner, the analogy between the vegetation and climate of certain regions, widely separated the one from the other, and point out in which cases new attempts at cultivation should be tried or where they should be discouraged. A celebrated geologist was able to say, beforehand, there is gold in such a part of New Holland; and gold was found there. We can also say, the olive tree and the cork oak will succeed in Australia; the eastern and temperate region of the United States is favorable to the growth of Chinese plants, more particularly

to that of tea; and we can assert that that part of America included between San Francisco and the Oregon territory will, one day, supply wines as varied and as excellent as those European ones produced between Portugal and the Rhine.

It is a singular fact, that the two principal beverages of the civilized world, wine and tea, which produce similar stimulating effects, but which to a certain extent are the substitutes one for the other in different countries, present also in the mode of cultivating them the most marked resemblances and differences. The vine and the tea-plant succeed best on stony, barren hillsides, of which they sometimes increase the value a hundredfold. According to the exposure, the soil, the cultivation and manner of preparing the produce, wine and tea are obtained of unquestionable excellence; while the neighboring crops, but a short distance off, may be more or less ordinary in quality. The two shrubs require a temperate climate, but the vine needs heat and no rain during summer, while the tea-plant requires rain and but little summer heat; the result of which is, that these two species are almost geographically incompatible. Vine-growing countries will never produce tea, and *vice versâ*.

But you will say, these examples belong rather to agriculture, and concern neither botany nor gardens. I maintain the contrary. It is science, in the present day, which points out what plants to cultivate, and into what countries to introduce them. Horticulture makes the trial, with infinite pains. If successful, the young plants are submitted to the less refined treatment of agriculture. Before the happy introduction of Cinchonas into British and Dutch India could be effected, botanists were required to collect, distinguish, and carefully describe the various species of American Cinchonas; horticulturists were then called on to make cuttings, gather the seeds, raise the young plants, transport and establish them in another part of the world; and so at last they were passed over to the care of the agriculturists. The coffee-plant did not spread gradually from Arabia to India, from India to Java; nor was it the American colonists who brought it from its original country to their fazendas or haciendas. The shrub was first described by botanists, and was afterwards introduced by the Dutch into a garden at Batavia; from thence it was taken to the Botanical Garden at Amsterdam, from whence a specimen was sent to the King of France in 1714. DeClieu, a naval officer, transplanted it from the garden at Paris to the French colonies in America. A multitude of such instances might be named. In the present day science has progressed, practical men avail themselves of it, governments and nations have abandoned those mistaken ideas in accordance with which it was supposed that a cultivation advantageous to one country was injurious to others. Hence we may hope to see,

before long, useful species planted in all regions where they can thrive, to the great advantage of mankind in general.

One of the most evident effects of science has been to create in the horticultural public a taste for varied and rare forms. Formerly in gardens there were only to be found certain kinds of plants which dated back to the time of the Crusades, or even of the Romans. The discovery of the New World did not produce a change in proportion to its importance; perhaps because horticulturists did not travel enough, or acquaint themselves with those countries whose species were most suitable for cultivation in Europe. Botanists, fortunately, were more ambitious. Their collectors were numerous and daring. They enriched their herbaria with an infinitude of new forms, and published works upon exotic plants, such as those of Hernandez, Rumphius, Sloane, &c. The immense variety in the forms of plants was thenceforth recognized, and in point of taste the elegant simplicity of the primitive flowers was able to vie with the gaudiness of the double ones. Then ceased the reign of tulips and pæonies in flower gardens. Curiosity, that great incentive to all science, having penetrated horticulture, the change in gardens became rapid. Instead of a few hundred species such as were cultivated at the commencement of the last century, there are now 20,000 or 30,000 to be found in most of the present catalogues. The single family of Orchids has probably more different representatives in our hothouses than was the case with all the families of plants put together, a hundred years ago. Fashion, united to the present curiosity of amateurs, causes, from time to time, old plants to be abandoned for new ones; and thus the entire vegetable kingdom will ultimately pass under the observation of civilized man.

What would horticulturists do, amidst this invasion of thousands of species, had not botanists devised convenient plans of classification and nomenclature? The families, genera, and species, have all been arranged in books, just as the districts, streets, and numbers of the houses are in our great capitals—with this superiority of method, that the form of the objects indicates their place—as if, in looking at a house in a town, one might discover, at a glance, to what street and what quarter it belonged. The plan of giving a single name to each species, besides its generic name, together with the prohibition of changing names without due reason, of giving the same appellation to two different species or two genera, far excels our plan of distinguishing individuals. How much it would simplify our intercourse with men, and facilitate our inquiries, if, in the whole world, the members of one family only bore the same name, and if each individual had but one christian name, differing from those of the other members of his family. Such is, neverthe-

less, the admirable plan of nomenclature that science has provided for horticulturists, and which they cannot too much appreciate and respect.[19]

3. *The beneficial effects of the association of botany with horticulture.*—The pursuit of horticulture demands books and herbaria, as that of scientific botany requires cultivated living plants. Thence the necessity, which is more and more recognized, of bringing together the materials for comparison in the same town, the same establishment, and even under the same administration, organized so as to facilitate the use of them. How many institutions in Europe, either private or public, would be benefited by this arrangement! How many towns and countries are now deficient—some in libraries, some in herbaria, some in respect to horticulture. Professional men proffer their complaint; let us hope that public opinion may end by listening to them.[20]

The bringing together the means of study, I have said, is desirable. Not less so is the interchange of ideas and impressions, both of botanists and horticulturists. Each of these classes must clearly have distinct characteristics; but the one should be influenced by the other. By these means, some too retiring dispositions may be brought out, and certain dormant powers developed. Horticulture, for instance, has a commercial tendency which may be carried too far. Charlatanism may slide in among flowers. Botany, on the contrary, is a science, and consequently rests on the investigation of pure and simple truth. A horticulturist who allows himself to be influenced by a scientific spirit necessarily frees himself from over-selfish tendencies. Natural history, on its side, by reason of the perfection of its method, its nomenclature and its minute observations, has something technical and dry about it, which contrasts with the grandeur of nature, and with the sentiment of art. It is for horticulture, combining, as it does, the planning and the decorations of gardens, to develop the æsthetic faculties of the savant, as of the world in general. A lovely flower, beautiful trees, a splen-

[19] Two years ago I made a request to the Fédération des Sociétés d'Horticulture Belges, which appears to have been favorably received, and it may not be useless to repeat it here. It consisted in begging the horticulturists who obtain new varieties not to give them botanical names, with a Latin designation, but merely arbitrary names of quite a different nature, in order to avoid confusion and useless researches in books. For example, if they called a Calceolaria, Sebastopol, or Triomphe de Gand, every one would understand it meant a garden variety; but if they named it Lindleyi, or mirabilis, one would think that it was a botanical species, and would search for it in scientific works, or in the Floras of Chili; and botanists, happening perhaps to mistake it, would add it to the end of a genus in their books as a species imperfectly known. The more horticultural names differ from Latin ones, the better it is, unless they can be appended to the scientific nomenclature: as when we say Brassica campestris oleifera, instead of, shortly, Colza.

[20] The Botanical Gardens at Kew are a fine example of what should be done, either on a large or a more modest scale, in many towns where the means of study are yet inconvenient or incomplete.

did floral exhibition, excite a sort of admiration, and even enthusiasm, similar to the effects produced by music or painting.

The powers of the German composers of modern days, and those of the Italian painters of the 16th century are justly extolled; but may it not also be said, that in point of art they are equalled in their way by the beautiful parks of old England? The feeling of harmony, in form and color—is it not also studied in them? The effect of contrast—is it not skillfully managed? The gradual transition from architectural to natural beauties—is it not treated in an admirable manner? Yes; decidedly the English landscape gardeners are poets; they have drawn from the same sources of inspiration as the most national writers of their country, and that source is the appreciation, so universal in England, of the beautiful, in an aspect of nature which is elegant and attractive, though somewhat grave.

Thus, gentlemen, for the development of our talents, as well as for our actual benefit, art and science keep pace together. Let us rejoice over their union, rendered conspicuous to-day by this congress of botanists, held in connection with a great floral exhibition; and after these general observations—perhaps rather too protracted—let us enter upon the consideration of those more truly scientific subjects, in which many among you are no doubt disposed to take part.

ART. XXXVII.—*Caricography;* by Prof. C. DEWEY.

Continued from vol. xli, p. 331.—1866. (The 43d No.)

No. 299. *Carex retrocurva*, Dew. 1845.

Spikes distinct; staminate single, terminal pedunculate cylindric; pistillate spikes 2–5, cylindric short-oblong rather close-flowered, often remote, leafy-bracteate long, and filiform-pedunculate pendulous or part resting on the earth, the lowest nearly radical; stigmas 3; fruit ovate or obtusish, short-rostrate, triquetrous, slightly nerved, about equaling its ovate cuspidate scale; culm 8–16 inches high, nearly erect, then subprostrate; leaves sub-radical, soft and wide; whole plant rather glaucous.

Open woods, Massachusetts and New York; south, north and west. When *C. oligocarpa* was confounded with *C. digitalis*, this was called by Dr. Gray *C. oligocarpa* var. *latifolia*, Gray, Gram. and Cyp., 1835, as quoted in Tor. Mon., p. 416, 1836.

No. 300. *C. stylosa*, Meyer. 1830.

Terminal spike erect, cylindric, short and staminate; pistillate spikes 2–3, often 2, short-cylindric, close-fruited, lowest leafy-bracteate and rather remote; stigmas 3; fruit ellipsoid triquetrous, stipitate, tapering above into a short beak with entire orifice, and exceeding the obtuse or ovate-oblong scale; culm scabrous above and leafy below; leaves linear, narrow and roughish on the margin.

Russian America—Unalaska, *Meyer*, and Sitka, *Bongard*. Contrary to the remark made in vol. xxix, p. 252, 1836, from Meyer's figure, this plant is not *C. Carltonia*, or even *C. Parryana*, Dew., but is far different. Mon. 1836.

No. 301. *C. Hartwegii*, Boott, Plant. *Hartwegianæ*. 1842.

Spike compound, 2–3 inches long; spikelets 5–7, oblong or short-cylindric, staminate at their summit, alternate, the upper approximate and sessile, the lowest sometimes compound, also pedunculate sub-remote and bracteate; stigmas 3; fruit oval or ovate-oblong, rostrate, sub-triquetrous bidentate, sub-scabrous on the margin above and nerved, a little exceeding its ovate or oblong lanceolate scale which is green on the pale back; culm 8 to 10 inches high, rather slender, not filiform, but leafy; leaves narrow, flat, often longer than the culm, slightly rough on the margin; plant pale green.

The *sterile, tumid and nerved scale between the spikelet and the axis at the base of the lateral spikelets*, noticed by Dr. Boott in this species and two others, is a very striking and curious character.

California, Dr. H. N. Bolander; Guatemala, *Hartweg*, says Dr. Boott in the above reference. From *C. Deweyana*, it differs in having *three* stigmas.

No. 302. *C. Davalliana*, Smith. 1800?

Flowers dicœcious with an oblong simple spike, never androgynous; fruit *distigmatic*, oblong-lanceolate, rostrate and roundish, tapering above and commonly much recurved, sub-scabrous above, nerved and longer than the ovate acute or awned scale; culm 5–8 inches high; leaves short, radical and filiform; both somewhat scabrous.

Rocky Mountains, Richardson; common in northern Europe. Separated from *C. dioica*, L., by the fruit, and from *C. gynocrates*, Wormsk., by being truly dicœcious, as that has stamens often on its fertile spikes, as well as wholly staminate spikes, and hence is described as androgynous and polygamo-dicœcious. Fries, Lang and Anderson notice these differences. The plants from the Rocky Mountains are exactly like *C. Davalliana* from Europe, and President Smith of the Linnæan Society, is adequate authority for the specific name.

No. 303. *C. Gayana*, Desv.

Spike composed of 4–8 spikelets aggregated into an ovate head; spikelets staminate above, ovate, sessile, and the lower sometimes branched, or staminate and pistillate spikelets closely aggregated (Boott), or sometimes dicœcious (Boott); stigmas two; fruit roundish ovate, short-acuminate or beaked, sub-scabrous above, shorter and narrower than the ovate-acuminate or broad ovate lanceolate or cuspidate scales; culm exceeding a foot in height and leafy toward the base; leaves narrow and long often as the culm, scabrous on the edge; all light green except the dark brown, and rusty-like spike.

Boundary Survey and Rocky Mountains, Fendler, 881, and Hall, of Ill.

Notes.—1. *C. fusiformis*, Chapman, in vol. vi, p. 244, 1848, has been cancelled by the author; but as it is an authentic form of *C. debilis*, it here receives the name, *C. debilis* var. *fusiformis*, Dew. It is described in the above reference.

2. *C. Oederi*, Ehrht., has occurred in a dicœcious form; numerous pistillate spikelets on one culm, and the staminate one to three short spikes on another culm, but both growing from the same root; about fifteen inches high.

Grand Isle, Judge Clinton; a singular form.

3. *C. viridula*, Mx., is a var. of *C. Oederi*, Ehrht., as Dr. Torrey learned from an examination of the herbarium of Michaux, and as stated in this Journal, xxvii, p. 276, 1835, and Tor. Mon., p. 417, 1836. New England, the state of New York, and Canada have given forms of *C. Oederi, short, small, with smooth culm: spikes three, the two lower axillary or bracteate and pistillate entirely, nearly or quite sessile, and the upper one staminate below and all small, while the triquetrous rostrate acuminate fruit allies it to C. flava* and not to *C. triceps*. It differs enough from *C. Oederi* in Michaux to have another name, but not since *C. Oederi* has been found so variable a species, and yet so alike itself. It differs even more from *C. triceps* and *C. hirsuta*. So accurate was Dr. Torrey in that early day in the determination of nearly all the species of Carex given by Michaux.

4. *C. Buxbaumii*, Wahl., *C. polygana*, Schk., (not Muh.) has very variable spikes, stigmas 3 in United States, and culm sharply triangular and very scabrous to partially smooth. The pistillate scales in Wahl., are said to be *cuspidate, about equalling the fruit*, and by others to be ovate, mucronate or cuspidate. On specimens from Germany the scales generally agree with the description, though much longer on some, and on specimens from Rhode Island the same, as well as from Canada W.; from the Rocky Mountains, with obtuse scales much shorter or much longer than the fruit; from Kentucky, Michigan, and New York, with scales *ovate lanceolate or ovate cuspidate, often twice longer than the fruit*, and black or dark; rusty on the sides as usual; culm very stiff and rough. From the marsh, Bergen.

5. *C. striatula*, Mx., 1803; xxvii, 278, 1835.
— *blanda*, Dew.; x, 45, 1826.

These two were found to be the same by Dr. Torrey, as he had access to the herbarium of Michaux, and both are described under the last reference; of the latter, its synonym, *C. conoidea*, Muh., given, while some botanists placed it under *C. anceps*.

6. *C. vaginata*, Tausch, xli, p. 227, 1866, and var. *alticaulis*, Dew.

Both of these forms have been abundant this season in the marsh at Bergen. The former is the shorter and has the larger fruit; the latter has much the more slender culm with narrow leaves. The latter has also been collected in great numbers at Belleville, C. W., by Macoun, with few of the former. The *refracted* culm above the upper pistillate spike has been uncommon this year at either locality.

ART. XXXVIII.—*Mineral Notices;* by CHARLES UPHAM SHEPARD.

1. *On Hagemannite, a new mineral from Arksutfiord, Greenland.*

FOR my knowledge of the present species I am indebted to Mr. G. Hagemann, chemist to the Natrona chemical works, Alleghany county, Pennsylvania, for whom it is named and from whom I received it, along with its associates, pachnolite, cryolite, etc.

The mineral is in seams and veins of from one-third to half an inch in thickness, generally having white cryolite closely adhering to its sides, though it sometimes traverses a drusy ferruginous pachnolite. It has in some instances the appearance of having been deposited in layers over broad undulating surfaces, when it resembles certain opaline deposits, as menilite. Its color is ochre- or wax-yellow, rarely with a faint tinge of green; and being impalpable in structure, dull (or only faintly glimmering) and opake, it reminds one of a very compact iron-flint, or of the yellow variety of chloropal from Alar, Bavaria. Its streak is paler than its color. It is not difficultly frangible, and shows an even fracture. H.=3·0 to 3·5. G.=2·59 to 2·60. It adheres but feebly to the tongue, without emitting a strong argillaceous odor.

When held in the flame of a candle, it decrepitates with surprising energy, throwing considerable fragments to a distance. In order to prepare it for blowpipe experiments, the mineral requires to be heated in a closed tube, during which it is observed to evolve much water and hydrofluoric acid. The decrepitated fragments soften easily in the first heat of the blowpipe, but without assuming a globular shape like cryolite. The color of the fused fragment passes quickly from light-pearl to a dirty greenish-gray. To borax it only imparts a feeble iron tinge. Its powder heated in a porcelain crucible to full ignition lost 10·1 p. c., and fused into a pinkish-white hard mass with a very rough surface. Having satisfied myself of the homogeneous nature of the mineral and its leading constitution, I requested Mr. Hagemann to undertake its analysis; and I here subjoin the results he has reached.

"In the specimen selected, (G. =2·83) it was impossible to discover even with the microscope, anything but the yellow mineral, which showed in many places a crystalline structure, or minute golden yellow points. It was hard to pulverize. The powder was heated with sulphuric acid in a platinum dish, whereby, with slight warmth, it evolved $SiFl_2$. When farther heated, it turned white, was easily decomposed, and by boiling with water and

chlorhydric acid gave a yellow solution. This, after filtering and oxydation by nitric acid, was precipitated with ammonia, much chlorid of ammonium having been previously added. The precipitate was redissolved and reprecipitated twice before it was free from lime. Lime was precipitated as usual, the filtrate evaporated to dryness and evaporated; the residuum redissolved and the soda and magnesia separated by means of acetate of baryta. Fluorine was determined by Wöhler's process, and silicium by boiling the mineral with carbonate of ammonia. The precipitated $\ddot{A}l_2+Si$ was separated as usual. Water was determined by heating the mineral under lime. The mineral contains a trace of phosphoric acid.

Al	Fe	Ca	Mg	Na	Fl	Si	HO	Insol.
12·00	5·82	11·20	2·30	8·45	40·10	7·79	10·44	1·08
12·21	5·87	11·16	....	...	40·51	....	....	....
11·98	6·17	11·16	....	....	....	....	....	....
Mean, 12·06	5·96	11·18	2·30	8·45	40·30	7·79	10·44	1·08

HO	= 10·44	$\frac{10·44}{9}$	=		1·16	or 2	HO
Al	= 12·06	$\frac{12·06}{13·6}$	= 0·886	= 1·098	"	2	Al
Fe	= 5·96	$\frac{5·96}{28}$	= 0·202				Fe
Ca	= 11·18	$\frac{11·18}{28}$	=		0·559	" 1	Ca
Mg	= 2·30	$\frac{2·30}{12}$	= 0·191	= 0·558	"	1	Mg
Na	= 8·45	$\frac{8·45}{23}$	= 0·367				Na
Fl	= 40·30	$\frac{40·30}{19}$	=		2·125	" 4	Fl
Si	= 7·79	$\frac{7·79}{14·8}$	=		0·526	" 1	Si

The deduction of a formula is difficult. The following is suggested:

$$2(Ca+\tfrac{2}{3}Na+\tfrac{1}{3}Mg)Fl+(\tfrac{3}{4}Al+\tfrac{1}{4}Fe)^2SiFl^2+2HO$$

but it is very complicated; and it is uncertain whether $SiFl_2$ is capable of combining thus with metals. The iron was found to be present as sesquioxyd."

2. *Cotunnite at South Hampton Lead Mine.*

In a recent number of this Journal I have described scheelite as a rare product of the Hampton lead mine. I am now able to add cotunnite (PbCl) as a production, though similarly scarce, of the same locality. Two or three specimens have been brought to me by one of my pupils (Mr. P. W. Lyman, of the Junior class in Amherst College); and I have since heard of a fourth specimen, found by another visitor of the mine. The crystals are small, and occur in groups lining druses of quartz. They have the form of right-rectangular prisms, are without transparency and perfectly milk-white. When reduced to a fine powder the mineral is soluble in water, from which the nitrate of silver throws down the chlorid of silver.

3. *Columbite at Northfield, Mass.*

This mineral was sent to me last autumn for determination by Mr. M. A. Brown, of Springfield, Mass., an enterprising mineralogist, now on his way to Montana. I visited the locality with him last month. It is on land belonging to Mr. Simeon Lyman, and situated about one mile northeast of the village. It occurs in a much disintegrated coarse-grained graphic granite, which here forms veins from ten to fifteen feet in width, traversing the micaceous schist. Beryl is also somewhat abundant in the vein, in crystals often several inches (up to ten) in diameter, of a pale greenish-white color, and generally peculiar by their uniform shortness or tabular form, and the regularity of their terminations by a single plane. In this respect they resemble the beryls of Goshen and Norwich. The columbite is tolerably crystallized, black and shining, with a specific gravity of 6·5, which, it will be observed, is much higher than that of the Connecticut localities and nearly identical with the Bodenmais variety. The largest fragments weighed only a few ounces; and the supply at the locality (which is mostly derived from the soil contiguous to the vein) is very limited.

Very interesting specimens of crystallized fibrilite in distinct white prisms having nearly the form of kyanite are occasionally met with in the drift of this region. The crystals penetrate a compact micaceous rock in all directions; and from their great hardness are found projecting at various angles quite beyond the surface, notwithstanding the attrition to which the masses have been subjected.

In the remote southeastern section of the town, on what is called Northfield mountain (on the road to Irving), the following minerals are frequent, viz., garnet, kyanite, epidote and beryl. Also at what has been called the Black-lead mine, on land of Mr. Piper, where a highly plumbagenous mica-slate has been a little worked with a view to plumbago, I noticed several interesting specimens of the astrophyllite variety of mica. Grace mountain in Warwick is visible to the northeast from this vicinity, and is the locality of the beautiful radiated black tourmaline associated with granular epidote.

4. *Spodumene in Winchester, New Hampshire.*

This is a continuation of the Goshen formation. The locality is on land of Mr. Brown (the father of Mr. M. A. Brown), a wheelright, whose house is just upon the line dividing Northfield from Winchester. The spodumene ledge comes into view directly adjoining his dwelling. The mineral does not exhibit distinct crystals, but only long bladed and easily cleavable masses, which form the bulk of the rock. It is chiefly interesting as our most northern locality of this species, and as a prolongation of the Goshen rocks.

ART. XXXIX.—*Brief Notices of several localities of Meteoric Iron;* by CHARLES UPHAM SHEPARD.

1. *Savisavik,*[1] *North Greenland.*

THIS meteoric iron has been in my possession upward of two years; and I had hoped, before describing its locality, to have obtained a supply of material fully adequate to its description and analysis; but not succeeding in this I deem it best to delay my notice of it no longer.

My specimens, consisting merely of a few scales, scarcely larger than one's finger nail, were the gift of John C. Trautwine, Esq., Civil Engineer, of Philadelphia, to whom they had been presented by Dr. Hays, the well-known arctic voyager, accompanied by the following note:

"* * I send you the fragment of iron (supposed to be meteoric) which I promised. It was obtained from an Esquimaux at my winter station of Port Foul, in 1861, who had obtained it at a place called Savisavik, and had carefully preserved it with a few other fragments, to make (with an ivory blade) the edge of a knife. The name of the place is derived from "savik," *knife*, and means the place where knife-material is found, i. e., iron place. The Esquimaux told me that there is a large mass of this material, and that the natives go there frequently to obtain it. The locality is about twenty miles south and east of Cape York, North Greenland, near latitude 76°. The Esquimaux scale off fragments with flint stone.

Yours, etc., I. I. HAYS."

Philadelphia, April 17, 1864.

This iron is perfectly malleable and remarkably homogeneous, without being much prone to oxydation. Its specific gravity is just below 8, which is rather high, but doubtless occasioned by the condensation it has suffered in being detached from the parent mass. For a time it was supposed that these fragments had been brought from the Niakornak locality in North Greenland; but a comparison of specimens fully disproves the idea. Some collectors have expressed doubts moreover, whether the last mentioned locality was a genuine meteorite. On this point, however, I have entertained no doubt, and am happy in being supported in this opinion by so high an authority as that of the late Prof. Forchhammer, who, in a letter to me dated Copenhagen, March 8, 1865, observes, "the Greenland (Niakornak) meteoric iron is certainly no artifical product, although it contains but little nickel and cobalt, and so much carbon, that it is hard and brittle like cast-iron.

[1] It is not plain from Dr. Hays's letter whether this word begins with S or G.

2. *Botetourt County, Virginia.*

This iron was discovered more than fifteen years ago in a mass so ponderous that the finder, having attempted to transport it on horseback a number of miles to his house, was obliged to abandon the undertaking. He left it upon a stone wall by the road-side, after having (with the assistance of a negro who happened at the time to be passing with a hammer,) detached two or three small angular fragments. These were afterward given to Mr. N. S. Manross, who took them with him to Göttingen, where in the laboratory of Prof. Wöhler he analyzed one of them so far as to determine the presence of nickel in the very unusually high proportion of more than 20 per cent. In the year 1860, while Mr. Manross was delivering lectures in this college on chemistry, he presented me a little fragment of this iron along with the foregoing information; and after his melancholy death at the battle of Antietam, his widow gave me the only remaining specimen of it that is known.

The quantity is too small to justify a further analysis; and I content myself with a brief description of its physical properties. It is whiter than most irons, extremely close and homogeneous, with exception of a few minute pyritic grains. Specific gravity =7·64. Fracture fine granular like cast-steel. It does not give the Widmannstätian figures. In composition and structure it resembles the Green county (Tenn.) iron. As it is a variety not prone to decomposition, it is to be hoped that the original mass may yet be rediscovered, although it is scarcely probable that either of the persons who were once acquainted with its position are still alive.

3. *Colorado.*

If neither of the two preceding irons are likely to be represented in our collections, there is certainly a prospect that it will be quite otherwise with the mass just discovered upon the eastern slope of the Sierre Madre Range of the Rocky Mountains.

For my acquaintance with this discovery I am indebted to the kindness of Mr. J. Alden Smith, a practical mineralogist, at present residing in Colorado. This gentleman has transmitted to me by mail a very interesting cleavage lamina, 1½ inches long by ⅔ths of an inch wide and ⅛th thick, and which shows on one edge a portion of the natural coating of the meteorite. His letter, dated June 21st is very brief, though it contains important particulars which I cannot withhold from the scientific public until his return to the east in the coming autumn. By means of the promised specimens he expects to bring with him on his return, I hope to be able to give a more circumstantial account of the discovery.

The detection of the mass, and which has occurred only within

a few weeks, is due to Messrs. Wilson and Morrison, by whom Mr. Smith was shown to the locality. It is situated within a very deep ravine, at the elevation of 8000 feet above the ocean and surrounded with high mountains on all sides. The exact dimensions of the mass are not given; but its weight is supposed to be several hundred pounds. "It seems to have struck a crevice in the solid ledge, and thereby to have been much shattered at one extremity,—a circumstance that enabled the finders to detach several small pieces." They inferred the fall to have taken place at a very remote period, as the mass exhibited a coating of oxyds half an inch thick. "Its composition is principally the native metals, iron, nickel, cobalt, a little manganese and a trace of copper. In some parts, iron forms the chief ingredient, while in others nickel and cobalt are largely in excess."

The specimen in my possession exceeds every iron I have seen in the perfection of its crystallization. It is as coarsely crystalline as that of Arva (Hungary) or Cocke county (Tenn.), but much more intimately laminated with Schreibersite than either. The laminæ of this substance are unusually thick, and possess a light color together with a bright luster. As they are disposed in accordance with the octahedral cleavage of the iron, they render the Widmannstätian figures strikingly apparent without polishing or the use of acids. No pyrites or graphite is visible in my specimen. Specific gravity = 7·43.

4. *Supposed new locality in Tennessee.*

Through the kindness of a scientific friend in Mississippi, Dr. W. Spillman, I am able to announce the very recent discovery of a considerable mass of meteoric iron upon a mountain in Tennessee. It was accidentally met with by a mining explorer, and is described as being "as large as a man can lift." The finder hammered off only a fragment of very small size which was forwarded to me by letter. Its original structure had been destroyed by the process of detaching the fragment. It was nevertheless highly malleable with the usual luster and color of meteoric iron. It was rapidly dissolved by chlorhydric acid without the odor of sulphuretted hydrogen. After being treated by nitric acid and ammonia, the characteristic purplish blue of the ammoniacal solution of Ni was exhibited. Measures have been taken to secure the mass, when a full description of it will be given.

Amherst College, July 9, 1866.

ART. XL.—*Appendix to Article XXX, On the Origin of some of the Earth's Features;* by JAMES D. DANA.

ON page 210 I have made but a bare allusion to the question of the heat required in metamorphism. Mr. Vose dispenses with heat altogether, except what may be incidental to compression. And Professor Hall regards it as of secondary importance, or not absolutely necessary (see our citation on page 207), and attributes the little extraneous heat that may be present and operative—probably, he says "not much above that of boiling water" (Pal., vol. iii, p. 77)—to the sinking of the thickening deposits to a level "where the surrounding temperature was higher;" higher, that is, on the principle, first suggested by Herschel, of the rising of the isothermal planes within the earth's crust in concordance with increase of thickness through superficial deposits; the isothermal plane of 100°, for example, being within a certain distance of the surface of the crust in a given region, and rising as the surface rises by new accumulations above.

The correctness of Herschel's principle cannot be doubted. But the question of its actual agency in ordinary metamorphism must be decided by an appeal to facts; and on this point I would here present a few facts for consideration.

The numbers and boldness of the flexures in the rocks of most metamorphic regions have always seemed to me to bear against the view that the heat causing the change had ascended by the very quiet method recognized in this theory. For the heat, thus slowly creeping upward, a few inches, feet, or yards in a century, should produce the change with little disturbance in the mass, and leave the beds nearly or quite horizontal: a condition very unlike that actually found in nature. The region of the thickened accumulations is also necessarily, as I have said, one of *strengthened* crust, under the gravity-hypothesis; and displacements, from any expansion of the crust which the slowly ascending heat might produce, should be mainly apparent in the surrounding regions where the crust had not been thus thickened.

But there are other facts indicating a limited sufficiency to this means of metamorphism. These are afforded by the great faults and sections of strata open to examination. In the Appalachian region, both of Virginia and Pennsylvania, faults occur, as described by the Professors Rogers, and by Mr. J. P. Lesley, which afford us important data for conclusions. Mr. Lesley, an excellent geologist and geological observer, who has explored personally the regions referred to, states that at the great fault of Juniata and Blair Cos., Pennsylvania, the rocks of the Trenton period are brought up to a level with those of the Che-

mung, making a dislocation of at least 16,000, and probably of 20,000, feet. And yet the Trenton limestone and Hudson River shales are not metamorphic. Some local cases of alteration occur there, including patches of roofing slate; but the greater part of the shales are no harder than the ordinary shales of the Pennsylvania Coal formation.[1]

At a depth of 16,000 feet the temperature of the earth's crust, allowing an increase of 1° F. for 60 feet of descent, would be about 330° F.; or with 1° F. for 50 feet, about 380° F.—either of which temperatures is far above the boiling point of water; and with the thinner crust of Paleozoic time the temperature at this depth should have been still higher. But, notwithstanding this heat, and also the compression from so great an overlying mass, the limestones and shales are not crystalline. The change of parts of the shale to roofing slate is no evidence in favor of the efficiency of the alleged cause; for such a cause should act uniformly over great areas.

In Southern Virginia, between Walker's Mountain and the Peak Hills, the Trenton rocks, as Lesley observes, are brought up, by means of a fault, to a level with the Lower Carboniferous. The amount of the fault by the lowest estimate is 15,000 feet. Notwithstanding the depth at which the Trenton beds had been lying previous to the faulting, the limestones are not granular marbles, but ordinary stratified limestone.

Again, in the great Nova Scotia section, at the Joggins, 15,000 feet of rock are exposed to view out of the 16,000 feet or more of the whole Carboniferous formation; and the lower strata of these 15,000 feet consist of shales and sandstones, and *fossiliferous* limestone, without metamorphism.

What is the natural inference from these data? Can we assume that the Coal formation in Rhode Island and Massachusetts, now in a high state of metamorphism, or the Devonian rocks of New England, now granite, gneiss, crystalline schists and marble, were once covered with deposits standing 15,000 feet above the present surface? As daring as this assumption would be, the condition would not give, as the facts show, the heat requisite for the metamorphism that has taken place.

We may say further: The pressure (Hall), or compression (Vose), from 15,000 feet of overlying rock, enormous though it be, added to the heat derived from below on the principle explained, and to the tension from expansion through this heat, and to any movements resulting from this expansion, is not sufficient to produce distinct metamorphic changes.

[1] In a recent conversation with Mr. Lesley, he confirmed these statements, and said that the upturned rocks are so situated that the approximate thickness of the series is easily ascertained. The facts are briefly alluded to in my Manual of Geology, on page 707.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On the chlorids of tungsten.*—DEBRAY has studied the vapor-densities of the volatile compounds of tungsten with chlorine, and with chlorine and oxygen, and has arrived at results of much theoretical interest. When a current of dry chlorine is passed over metallic tungsten heated to redness in a tube of hard glass, red vapors are obtained which condense to a dark gray mass, which is a mixture of the two chlorids WCl_3 and W_2Cl_5. By distillation in a current of chlorine the terchlorid WCl_3, may be obtained very nearly pure. There are, as is well known, two oxychlorids of tungsten corresponding to tungstic acid, the formulas of which are respectively WO_2Cl and $WOCl_2$. Debray obtains these bodies easily by Gerhardt's method by distilling the terchlorid with dry oxalic acid. The red oxychlorid or, as we should prefer to term it, oxydichlorid, $WOCl_2$, is easily obtained pure, but the yellow or dioxychlorid, WO_2Cl, is always mixed with tungstic acid or the oxydichlorid, as it is easily decomposed by heating, according to the equation

$$2WO_2Cl = WOCl_2 + WO_3.$$

The terchlorid heated with tungstic acid acts upon it with evolution of heat, according to the equation,

$$WO_3 + 2WCl_3 = 3WOCl_2.$$

The easy decomposition of the dioxychlorid makes it impossible to determine the density of its vapor, but the vapor-densities of the two other compounds may be readily taken in the vapor of mercury or sulphur, since the least volatile boils at about 300° C. In this manner the terchlorid gave in the vapor of mercury the density 11·50, and in the vapor of sulphur 11·89, 11·80 and 11·69. The oxydichlorid gave in the vapor of mercury 10·78 and 10·70, and in the vapor of sulphur 10·27.

If we admit that the formulas of these bodies correspond to 2 or 4 vols. of vapor, we find by calculation that the theoretical density of the terchlorid is 13·75 on the hypothesis of 2 vols., and 6·875 on the hypothesis of 4 vols., and that the vapor-density of the oxydichlorid is 11·86 on the hypothesis of 2 vols., and 5·93 upon that of 4 vols. If we admit with Perzoz that tungstic acid is $W'O_5$ and the perchlorid $W'Cl_5$, the equivalent of the chlorid becomes five-thirds of the old equivalent and its vapor-density five-thirds of that found above theoretically, so that if $W'Cl_5$ corresponds to 4 vols. of vapor we have for its vapor-density 11·46, which agrees nearly with that found by experiment. We must then suppose, however, that the oxydichlorid is $WO\frac{5}{3}Cl\frac{10}{3}$, when the calculated density upon the hypothesis of 4 vols. becomes 9·87. And as fractions of equivalents cannot be admitted we must write this formula $W'O_5 + 2W'Cl_5$, which is the same as admitting the existence of bodies the vapor-density of which corresponds to 12 volumes.—*Comptes Rendus*, lx, 820. W. G.

2. *On the separation of cobalt from nickel.*—TERREIL has given a method of separating cobalt from nickel which promises to yield good results. To the solution containing the two metals ammonia is to be

added in excess so as to dissolve the oxyds. To the hot liquid a solution of hypermanganate of potash is to be added until present in excess, as shown by the violet color of the solution remaining for a short time. The whole is then to be heated to boiling for some minutes and then a slight excess of chlorhydric acid added so as to dissolve the precipitated oxyd of manganese. The liquid is then to be kept for twenty or twenty-five minutes at a gentle heat and allowed to stand for twenty-four hours. All the cobalt is then deposited in the form of a crystalline powder of a beautiful reddish violet color (chlorid of purpureocobalt), which is to be thrown upon a weighed filter, and washed in the cold at first with dilute chlorhydric acid or a solution of sal-ammoniac, and afterward with alcohol. The filter and salt may then be dried at 110° C. and weighed. Or a known weight of the salt may be reduced in a current of dry hydrogen when pure cobalt will remain. The filtrate containing the nickel is to be boiled to expel alcohol, supersaturated with ammonia and hypermanganate of potash added. On boiling the whole of the manganese will be thrown down, while the filtrate contains the nickel. An alkaline hypochlorite may be used in place of the hypermanganate to oxydize the cobalt, but in this case the deposition of the chlorid of purpureocobalt requires several days to be completed.—*Bull. de la Soc. Chimique de Paris*, Feb. 1866, p. 88. W. G.

3. *On a new alcohol in which carbon is partially replaced by silicon.*—Friedel and Crafts have succeeded in replacing carbon by silicon by a somewhat circuitous process of the greatest interest in a theoretical point of view. Chlorine acts upon silicium-ethyl, $Si(C_2H_5)_4$, to form two products of substitution, monochlorinated and dichlorinated silicium-ethyl. These products cannot be separated by distillation, but when the mixture of the two, boiling between 180° C. and 200° C., is heated in a closed tube with acetate of potash and alcohol, the bichlorinated compound is first attacked, while chlorid of potassium is formed and the monochlorinated compound remains among the products of the action. When water is added to the contents of the tube after the action, an oily liquid separates which is to be washed twice with water and then treated with concentrated sulphuric acid, which dissolves the acetic acid compound and the oxyd of silicium-triethyl, $\left.\begin{matrix}Si(C_2H_5)_3\\Si(C_2H_5)_3\end{matrix}\right\}O$, leaving the silicium-ethyl and its chlorine derivatives unacted upon. The portion undissolved is to be washed, dried and distilled. The greater part passes over at 180°–190°, and is treated as before in a closed tube with acetate of potash and alcohol. The liquid separated by water is again treated with sulphuric acid, the solution decanted and poured into water. A liquid separates which boils between 208° and 214° C., has a faint ethereal and acetic smell, and burns with a luminous flame, giving off white fumes of silicic acid. This liquid has the formula $\left.\begin{matrix}SiC_6H_{10}\\C_2H_3O\end{matrix}\right\}O$, and is derived from monochlorinated silicium-ethyl by replacing the chlorine by oxacetyl, C_2H_3O. Treated with an alcoholic solution of caustic potash this body yields a new liquid boiling at 190° C., and having the formula $\left.\begin{matrix}SiC_6H_{10}\\H\end{matrix}\right\}O$, which is the hydrate corresponding to the acetate above

described. The authors term the radical, $Si\mathfrak{C}_8H_{19}$, *siliconyl*, and compare the hydrate and acetate to the corresponding compounds of carbon and hydrogen, $\left.\begin{matrix}\mathfrak{C}_9H_{19}\\H\end{matrix}\right\}$ and $\left.\begin{matrix}\mathfrak{C}_9H_{19}\\\mathfrak{C}_2H_3\Theta\end{matrix}\right\}\Theta$, considering silicium to replace carbon atom for atom.—*Comptes Rendus*, lxi, 792. W. G.

4. *On a new class of organic radicals containing metals.*—Berthelot has described a very remarkable series of bodies derived from acetylene, C_4H_2 or $\mathfrak{C}_2H_2$, and containing copper, silver, &c., in place of the hydrogen of the primitive radical. The first of these bodies, *Cuprosacetyl*, has the formula $C_4\mathfrak{C}u_2H$; its oxyd, $(C_4\mathfrak{C}u_2H)O$, is obtained by precipitating the ammoniacal subchlorid of copper by acetylene and washing the precipitate by decantation with strong ammonia. The oxyd is a reddish brown flocky precipitate, decomposed by chlorhydric acid with formation of subchlorid of copper and acetylene. It decomposes salammoniac solution when boiled with it, and is itself decomposed with difficulty by boiling with sulphurous and sulphuric acids.

The chlorid of cuprosacetyl is obtained by passing acetylene, bubble by bubble, into a concentrated solution of subchlorid of copper in chlorid of potassium. The gas is absorbed and a yellow precipitate is formed, which soon becomes crystalline. This precipitate, washed by decantation with a saturated solution of chlorid of potassium, becomes orange, then purple, and finally dark red. The precipitate is finally washed with water, when the chlorid remains pure. It is decomposed by ammonia with formation of the oxyd, and by boiling chlorhydric and nitric acids. It unites with chlorid of ammonium to form a double salt of a darker color than the corresponding potassium compound. The bromid and iodid of cuprosacetyl may be obtained by similar methods. The iodid has a magnificent vermillion-red color, and is much more stable than the chlorid and bromid. It closely resembles iodid of mercury but is insoluble in iodid of potassium. It forms an orange-yellow double salt with iodid of potassium, and also an oxyiodid. The author has also prepared a yellow oxycyanid and a basic sulphite of cuprosacetyl. The sulphid is obtained by the action of sulphuretted hydrogen water upon the oxyd, and is mixed with subsulphid of copper. Allylene gives similar compounds with the subsalts of copper; the chlorid and iodid are yellow.

The argentic compounds of acetylene are analogous to those of copper. They may be deduced from the radical C_4Ag_2H, which the author terms argentacetyl. The oxyd of this radical may be obtained by treating acetylene with nitrate or any oxysalt of silver dissolved in ammonia, and washing the precipitate first with ammonia and then with distilled water. It is the compound already known as acetylid of silver, and has the formula $(C_4Ag_2H)O$. The chlorid of this radical is a white curdy precipitate resembling chlorid of silver. The sulphate is a grayish-white substance decomposed by chlorhydric acid. The phosphate is a yellow curdy precipitate. Berthelot remarks that these compounds are the first known organic radicals containing copper or silver, and that in their mode of formation they differ from the radical resembling the ammonia-metallic bases of Gros, Reiset, &c., in being formed by the direct action of a hydruret upon a metallic salt. The analogy between cuprosacetyl and ammonia may be shown by the following formulas:

NH_3	C_4H_2	C_4Cu_2	C_4CuH	C_4AgH
NH_4	$C_4H_2.H$	$C_4Cu_2.H$ or	$C_4CuH.Cu$	C_4Ag_2H
NH_4O	....	C_4Cu_2HO or	$C_4CuH.CuO$	C_4Ag_2HO

The author further remarks that $(C_4CuH.Cu)O$ and $(C_4AgH.Ag)O$ are analogous to the oxyd of Reiset's base, $(NH_3Pt)O$, and that various facts lead him to believe that there are compounds analogous to the base $(2NH_3Pt)O$, such as $[(C_4AgH)_2Ag]O$.

In a subsequent paper the author describes similar compounds containing gold and chromium, the constitution of which, however, is not yet clearly ascertained. Silver unites with allylene to form argentallyl, the chlorid of which has the formula $[C_6H_3Ag(C_6H_3Ag_2)]Cl$, so that the radical corresponds to the second series of acetylene compounds above mentioned. When metallic sodium is heated in acetylene the gas is readily absorbed and a compound is formed having the formula C_4HNa, while the hydrogen set free unites with another portion of acetylene and forms ethylene, C_4H_4, and its hydruret, C_4H_6. Potassium acts in a similar manner but with more violence. At a higher temperature sodium replaces all the hydrogen and form C_4Na_2. The results given are to be considered as preliminary to a fuller investigation of the subjects.—*Bulletin de la Société Chimique*, March, 1866, pp. 176, 182. W. G.

5. *Isomerism.*—Berthelot, in a memoir on a new kind of isomerism, proposes the following subdivision of this subject. Isomeric bodies—that is to say, bodies formed of the same elements united in the same proportions—can be separated into a certain number of classes or general groups:—

(1.) *Equivalent composition.*—Substances which appear to have a purely accidental relation to each other; for instance, butyric acid $C_8H_8O_4$ and dialdehyde $(C_4H_4O_2)_2$.

(2.) *Metamerism.*—Bodies formed by the union of two distinct principles, so that in their formulæ a kind of compensation is established; for example, methylacetic ether, $C_2H_2(C_4H_4O_4)$ and ethylformic ether, $C_4H_4(C_2H_2O_4)$.

(3.) *Polymerism.*—Compounds arising from the union of several molecules to form one; this is shown in the case of amylene $(C_{10}H_{10})$ and diamylene $(C_{10}H_{10})_2$.

(4.) *Isomerism, properly so-called.*—There are bodies that, differing in their properties, retain these distinctive features in their passage through certain compounds, the properties of which result from the internal structure of the compound molecule taken as a whole, rather than the diversity of the components which have produced it. This is observed in the cases of essence of terebenthine and citron, the sugars, the symmetrical tartaric acids, and the two classes of ethyl-sulphates.

(5.) *Physical Isomerism.*—By which is meant the different states of one and the same body, the diverse nature of which vanishes when the substance enters into combination. To these five classes, Berthelot proposes to append a new one, called kenomerism (from κενὸν), distinct from all the others, though allied to metamerism.

(6.) *Kenomerism.*—Two different compounds may lose, by the effect of certain reagents which bring about decomposition, different groups of elements, and the remainders be identical in composition; these two de-

rivatives, however, may yet be distinct the one from the other both in physical and chemical properties. They retain to some extent the structure of the compounds from which they take their origin. To take examples: alcohol by losing 2 equivalents of hydrogen is turned into aldehyde: $C_4 H_6 O_2 - H_2 = C_4 H_4 O_2$.

Glycol, on the other hand, by giving up 2 equivalents of water, is converted into glycolic ether (oxyd of ethylene):

$$C_4 H_6 O_4 - H_2 O_2 = C_4 H_4 O_2.$$

Glycolic ether and aldehyde are isomeric; their composition is the same, but their properties, both physical and chemical, are extremely different. This is a good case of kenomerism. Again, essence of terebenthine combines with hydrochloric acid under different conditions to form *two* distinct hydrochlorates, the monohydrochlorate, $C_{20} H_{16} H Cl$, and the dihydrochlorate, $C_{20} H_{16} 2H Cl$. From the first body the crystalline compound $C_{20} H_{16}$, camphene, is obtained, and from the latter $C_{20} H_{16}$, terpilene, two hydrocarbons of very different properties.—*Reader*, July 7.

6. *On a new determination of the velocity of sound in different media.*—August Kundt has, by a course of experimental investigations performed in the laboratory of Magnus of Berlin, arrived at new and very interesting results in regard to the longitudinal vibrations of gases, and disclosed a new method for the determination of the velocity of sound in gases and solids, which gives as accurate results as any other method, and besides is admirably adapted for the class-room.

After having enlarged our knowledge of longitudinal vibrations of glass tubes coated on the inside with lycopodium, Mr. Kundt closed one or both ends of the longitudinally vibrating glass tube; instead of the accumulations observed by Savart he found the lycopodium to form a beautiful regular wave-line with transverse ripplings, varying according to definite variations in the circumstances of the experiment.

Take a glass tube about four feet long and three-fourths of an inch in diameter, shake some lycopodium into the same so as to make it adhere like dust to the walls of the tube, close each end by a cork, hold the tube in the middle, and cause it to vibrate longitudinally; then there will always be 16 heaps of the lycopodium. The velocity of sound in glass being about 16 times as great as in the air, in the tube the distances between the heaps, produced by the stationary waves are corresponding parts of the wave-length of the tone in glass and air (here one-half wave-length). This number is therefore found to be independent of the dimensions of the glass-tube; Kundt has used tubes of from one foot long and one-twelfth of an inch diameter, to six feet long and three inches diameter. If the glass tubes vibrate with two nodes, there are always 32 heaps; with three nodes there are 48 heaps,—the distance between the heaps being always one-half wave-length for air; but the glass tubes when held in the middle give one-half wave-lengths, that is, when vibrating with two nodes one wave in glass, when with three nodes $\frac{3}{2}$ waves in glass, thus giving $\frac{1}{2} : 16 = 1 : 32 = \frac{3}{2} : 48$.

When the tube is held in the same manner, that is, when its length is the same part of a glass-wave, *the distance of the heaps* (half-wave lengths in the gas) *will be proportional to the velocity of sound in the gas*, or the number of heaps will be inversely proportional to that velocity.

For tubes filled respectively with air, carbonic acid, illuminating gas and hydrogen, Mr. Kundt obtained respectively 32, 40, 20, and 9 heaps, from which the velocity of sound (air = 1) is for carbonic acid $\frac{32}{40} = 8$, illuminating gas $\frac{32}{20} = 1{\cdot}6$, hydrogen $\frac{32}{9} = 3{\cdot}56$. Dulong found, by a very difficult method, for carbonic acid ·79, for hydrogen 3·8

To obtain still greater accuracy, and also determine the velocity of sound in different solids, Kundt closes one end of the glass tube by a cork, movable by means of a wire; while the other end is closed by a perforated cork, enclosing a rod of the solid submitted to the experiment. This solid rod has one-half of its length in the glass tube, which itself is somewhat longer than the entire rod. This rod is set in vibration.

It will easily be seen, that for the same mode of vibration the velocity of sound in the solid will be directly proportional to the length of the rod, and inversely proportional to the distance of the lycopodium heaps in the glass tube.

With a brass rod 941·5 mm. long and 5 mm. diameter, Mr. Kundt obtained, in three different experiments, in each making numerous measurements of the distances, the velocities 10·87, 10·87, 10·86. Another brass rod gave 10·94 and 10·90. Similarly for steel, 15·345, 15·334 and 15·343; for glass, 15·24, 15·25 and 15·24; for copper, 11·960.

Wertheim found for cast-steel, 14·961; for steel wire, 15,108; for copper, 11·167.

The above leaves no doubt that Mr. Kundt has enriched science with a *new method* for the determination of the velocity of sound in solids, gases and vapors, alike excellent for a high degree of accuracy in its numerical determinations, ease of execution, elegance and simplicity, making it exceedingly convenient for lecture experiments.

We are engaged in experiments to try the application of this method to liquids.—*Poggendorff's Annalen*, 1866, cxxvii, 497–523; *l'Institut*, 1866, p. 15–16; *Cosmos*, 1866, iii, 98–100. G. H.

7. *The vapor of water not absorbent of much radiant heat.*—Tyndall and Frankland have, on the basis of some experiments, ascribed to watery vapor an excessive absorptive power for heat. The former even says: "Comparing a single molecule of aqueous vapor with an atom of ether of the main constituent of our atmosphere, I am not prepared to say how many thousand times the action of the former exceeds that of the latter." (Lecture on Radiation, Sect. 12.)

Magnus has objected to these experiments because they did not insure the absence of *condensed* vapors; he has now succeeded in constructing an apparatus which affords positive proof of the presence or absence of condensed vapor, "fog." He has found that the *radiation* (which is proportional to the absorption) of the following gases and vapors gave the following deflections with his very delicate thermo-multiplier, all the gases being heated about to 230° C.: dry atmospheric air 3 mm.; air having passed through water 3 to 5; dry carbonic acid gas 100 to 120; common illuminating gas, about the same; air having passed through boiling water, irregular, but maximum deflection only 20, and only gradually increasing to this amount, while carbonic acid and illuminating gas produced the deflection *suddenly*. When the water boiled so strongly that fog became visible at the radiating point, the deflection was above 100.

From the circumstances attending the deflection of 20 mm., even this may be ascribed to the presence of fog.

Magnus, as well as Dove, Quincke, Riess, Kundt, and others who witnessed the experiments, have seen that air passed through water at the common temperature never gave a greater deflection than 3 mm.; when saturated at a higher temperature never greater than 20 mm.; and only when fogs appeared the deflection became about as great as with carbonic acid gas, viz., 100 mm.

Magnus also experimented with a number of other vapors. He also shows how the phenomena of *dew* are in accordance with his view; that dew would be impossible if watery vapor had so great an absorptive power as Tyndall supposes; but that all the deductions of Tyndall and Frankland in regard to climate and the glacial period remain true if we substitute fog or foggy vapor for true uncondensed vapor; and finally, that the aqueous absorption lines in the spectrum observed by Cooke and Secchi are contradictory to any extraordinary absorptive power in actual vapor.—*Poggendorff's Annalen*, 1866, cxxvii, 613–624.

G. H.

8. *Solar spots influenced by solar refraction.*—In a certain sense the observations of Carrington (this Journal, xxxviii, 142) and of Spörer have thrown the subject of the physical constitution of the sun back into uncertainty and doubt. But it seems that as little as Kirchhoff's observations upset our views of the constitution of the Laterna mundi of Copernicus, so also the remarkable observations above referred to seem rather destined to confirm than to destroy the more ancient hypothesis of several atmospheres of the sun; for Mr. Dauge, of the Academy of Brussels, has shown how all the striking phenomena observed by Carrington and Spörer may be fully accounted for by the refraction of the emergent rays in the atmosphere exterior to the photosphere of the sun. By a very simple elementary process Mr. Dauge demonstrates that such an atmosphere by its refraction will produce the following effects: 1st, augment the apparent diameter of the sun; 2d, augment the mean period of rotation of the sun; 3d, retard the apparent motion of a spot in proportion as the same recedes from the center toward the rim of the sun; 4th, the apparent period of revolution of a spot increases with its solar latitude; 5th, the solar refraction produces an apparent motion of the spots in latitude (the latitude decreasing from the eastern rim to the middle, and increasing from the middle to the western limb of the sun).

Taking the horizontal refraction of the exterior atmosphere less the apparent diameter of the sun at 25°, and neglecting some insignificant terms, Mr. Dauge obtains the following value of the period of revolution of a spot at a solar latitude λ

$$4\tau' = 25{\cdot}30^{\text{days}} \times \frac{90° + \beta}{115°}$$

where $\beta = \frac{\sin 25°}{\cos \lambda}$.

Taking the mean of Carrington's observations for every fifth degree of latitude, Dauge gives the following comparison between the observed (O) and calculated (C) values of the period of revolution expressed in days:

λ	0°	5°	10°	15°	20°	25°	30°	35°	47½°
O	25·30	25·11	25·20	25·51	25·73	25·90	26·34	26·92	27·96
C	25·30	25·33	25·38	25·50	25·68	25·92	26·22	26·80	28·33

Toward 45° the observations are very scarce, hence of little weight.

The agreement of these numbers is sufficient to prove that this refraction is the principal cause of the phenomena observed by Carrington and Spörer. It may be well to remember that Secchi had suggested the influence of solar refraction some time before the publication of Dauge's work.—*L'Institut*, 1866, pp. 159, 165–168. G. H.

II. MINERALOGY AND GEOLOGY.

1. *Geological explorations in Northern Mexico;* by A. RÉMOND. Compiled from his notes, and prepared for publication, by J. D. WHITNEY. 18 pp. 8vo. San Francisco, 1866.—We cite a few paragraphs from this valuable report on the geology of Northern Mexico.

"The mountainous region comprising the central and western portion of Northern Mexico belongs to the four states of Durango, Chihuahua, Sinaloa, and Sonora. Considering how celebrated this portion of Mexico has become for its mines and metalliferous veins, and how much has been written about it, it is surprising how little exact information has hitherto been obtained with regard to either its geography or geology. On comparing the principal published maps of the region in question, it will be seen at once how much they differ from each other in their delineations of even its main topographical features, while the details are entirely wanting.

"The name of the 'Sierra Madre' is usually applied to the main range of mountains of this country, or the western border of the plateau which stretches north through the territories of the United States, forming what may be called the great orographical feature of the continent. In northwestern Mexico this crumpled border of the great plateau comprises an extensive mountainous region, by no means forming a continuous single chain, but rather several central ranges, with associated groups of parallel ridges, all having the same general course, which is approximately north-northwest, and south-southeast. As the breadth of the chain widens as we go toward the north, so, too, that of the valleys increases in that direction, the whole system of mountains and valleys spreading out in something like a fan shape.

"Going north, the chain appears to sink gradually, although determinations of altitude in northern Mexico are extremely few in number. It is certain that there is, in about latitude 32°, a depression of the mountain ranges which extends entirely across the continent, and which would enable the traveler to cross from the Atlantic to the Pacific, without necessarily surmounting any elevation greater than four thousand feet.[1] The southeastern range is the highest, and the culminating point is said to be the Cerro de Cuiteco, sixty leagues northeast of Jesus Maria, on the western border of Chihuahua. The approximate altitude of the Cumbre de Basascachic is 7429 feet, and that of Guadalupe y Calvo, 7825 feet.

[1] See Emory, in Mexican Boundary Report, vol. i, p. 41.

To the north, the ranges east of Sahuaripa are also very high; but they have never been measured. No peaks or ridges, however, in this portion of Mexico attain anything like the elevation of the higher portion of the Sierra Nevada, few if any points exceeding 10,000 feet in altitude.

"The direction of the Sierra is nearly that of a line connecting some of the best mining districts in Mexico, which are situated on or very near the summit of the mountains. These districts are the following, enumerating them in their geographical order from the south toward the north: In Durango, San Antonio de las Ventanas, Guarisamey, and San Dimas, remarkable for their auriferous silver ores, and sixty-two Mexican leagues northeast of Mazatlan; in Chihuahua, Guadalupe y Calvo, and San Pedro de Batopilas, yielding fine specimens of native silver; also, Jesus Marie, in the same State, and the Real del la Cieneguita, Sonora, with silver and gold mines.

"The geological structure of the occidental slope of the Sierra Madre, as well as that of the other parts of this great chain, is exceedingly interesting, and, as yet but little known, notwithstanding the valuable investigations of Humboldt and other eminent men; for, up to the present time, the age of the different formations has never been fixed with any degree of accuracy, from want of materials and of sufficient observations. In 1863, 1864, and 1865, however, I explored quite a number of localities in northwestern Mexico, and was thus enabled to obtain a pretty good general idea of the geology of that region; and, in Sonora, to which my attention was especially devoted, I succeeded in finding fossils in sufficient quantity to allow of the determination of the age of the principal formations of the northern Sierra Madre. By tracing the connection of these rocks with those of Central Mexico, additional light will be thrown on those districts of which, at present, but little is definitely known.

"The igneous rocks, which occur more abundantly on the Pacific slope, are granites, either fine or very coarse-grained; porphyries, more or less feldspathic; and greenstones, all of which are cut by numerous dikes of extremely varied character. The granites, however, are very poor in veins of the precious metals, while the porphyries are highly metalliferous. In Sinaloa (Candelero) and Durango (San Dimas) we see that the granites underlie the metalliferous porphyries, and that the greenstones, in Sonora (near Hermosillo and in the vicinity of La Haciendita), penetrate through them.

"The oldest sedimentary rocks, which I have observed, belong to the Carboniferous series; this is represented in the eastern part of Sonora, by heavy masses of limestone, forming very high and rugged ridges, running a little west of north. The upturned strata are seen, in many places, to rest on granite. Argentiferous veins occur throughout this formation.

"The next group of sedimentary rocks, in order, is the Triassic; this forms isolated mountain groups in Sonora, and offers an interesting field for investigation. Instead of limestones, it is made up of heavy beds of quartzites and conglomerates, with coal-bearing clay shales; all of these are disturbed and elevated, and rest on greenstones, feldspathic porphyries, or granite. Wherever metamorphosed, the Triassic rocks are aurif-

erous and contain veins of silver ores. The metamorphic slates and limestones of the Altar and Magdalena districts, which include the richest gold placers of Sonora, may possibly be of Triassic age; but the fossils collected are too imperfect to admit of this being determined. There are some reasons for believing those rocks to be rather of Jurassic than of Triassic age, as they differ in lithological characters from both the Triassic and Carboniferous of northern Mexico, resembling, rather, the Jurassic gold-bearing slates of the Sierra Nevada, in California; besides, they lie outside and to the west of the Sierra Madre. It may also be noticed that the gold which they furnish does not resemble that obtained from the Triassic strata.

"The Cretaceous period is also represented at the foot of the Sierra Madre, at Arivechi, in Sonora. The strata belonging to this series are chiefly argillaceous shales, and they rest upon porphyries and Carboniferous limestone. They have been disturbed and elevated since their deposition. The fossils, which they contain in great number and in a fine state of preservation, will be noticed farther on.[2]

"All the above-mentioned formations were already in existence before the first eruption of the volcanic rocks took place. These latter are found scattered along the whole Pacific coast, and extend from the Gulf of California up to the very summit of the Sierra. It is very interesting to see the volcanic formations spread over so extensive a region, especially as there are no active volcanos known in northern Mexico, and not even any indications of ancient craters or vents.

"The lithological character of the eruptive materials is extremely varied, and there seem to have been several periods of igneous action preceded by as many disturbances of the strata, all of which took place after the close of the Cretaceous epoch. Three different series of volcanic rocks may be observed in Sinaloa and Sonora, unconformable with each other: and these again may be subdivided into groups, after a thorough examination has been made of the extensive suite of specimens which has been collected. The lower or oldest series affords several hundred varieties of porphyries, characterized by crystals of feldspar or augite. There are also very peculiar trachytic rocks, resembling granite in appearance. These volcanic materials occur in beds or in masses, and are frequently cut by dikes; but they are quite destitute of veins containing gold or silver, the only metalliferous ores they contain being those of copper (?) and iron, and these in small quantity. Various volcanic ridges in Sonora belong to this class. The second series consists of extensive beds of micaceous, trachytic tufas, and breccias, all more or less uplifted since their deposition, and covering the different igneous and sedimentary formations as well as the older volcanic porphyries. These attain a great thickness, between San Dimas and San Ignacio, in Durango and Sinaloa.

"Above these formations occur ancient alluvial deposits, with bones of

[2] "Several species have been identified by Mr. Gabb as already described from Texas, and figured by Roemer in "Die Kreidebildungen von Texas;" these are, *Ammonites pedernalis* von Buch, *Natica pedernalis* Roem., *Turritella seriatim-granulata* Roem., *Gryphæa navis* Hall, *Cyphosoma Texanum* Roem., *Eulima Texana* Roem. Besides these, two other species are identified, viz.: *Cardium multistriatum* Shum., and *Turbinolia Texana* Con."—*Report*, p. 11.

extinct animals (elephants) at two localities: near La Noria, northeast of Mazatlan, and in the Arroya de la Palma, two leagues east of La Casita, in Sonora.

"Sheets of basaltic lavas, somewhat similar to those of California, and probably of the same age, forming with tufas the upper volcanic series, overlie the other formations, occupying a nearly horizontal position.

"The most recent formation is that of the terrace deposits of sand and gravel, which occur in Sonora."

The pamphlet, after giving details on these several formations, closes with a list of the principal mines of northern Mexico, in which the ores they yield are mentioned, the dip and strike of the veins, and other particulars of interest.

2. *On Fucoids in the Coal Formation;* by Leo Lesquereux. 14 pp. 4to, with a plate. From the Trans. Am. Phil. Soc., xiii, 313.—After remarks on the very great rarity of Fucoids, or remains of any *marine plants*, in the Coal formation, Prof. Lesquereux describes a species found on Slippery Rock Creek, opposite Wurtemberg, Lawrence Co., Pennsylvania. The specimens were from the lower surface of a thin layer of limestone, immediately above a bed of coal 6 to 18 inches thick. The limestone, 1 to 1½ ft. thick, is overlaid by shales and sandstone 20 ft.; above this, fire-clay 2 ft.; limestone 3 ft.; shales 150 ft., containing some thin beds of stigmaria fire-clay, and in some parts branching cylindrical Fucoids, resembling *Palæophycus tubularis* Hall; micaceous sandstone, with a few remains of Palæophycus.

The frond, in the new species, is somewhat lyre-shape in outline, but varies much. It is 2 in. to 1 ft. in length, and half this in breadth. It has a fleshy margin from an eighth to a fourth of an inch thick; and it is crossed by curving ribs, which pass from the inner to the outer edge, nearly concentric with the lower margin.

This species is referred by Lesquereux to Sternberg's genus *Caulerpites*, and named *C. marginatus*. It resembles somewhat *Fucoides Cauda-Galli* of the Devonian,—Hall's *Spirophyton*—which Lesquereux refers to the same genus. It has nothing of the spiral character of the *F. Cauda-Galli*, on which Hall bases his name *Spirophyton*. Lesquereux considers this character not of generic value, and due only to a twisting of the frond as it grows—a peculiarity observed in some living Fuci.

Lesquereux closes his memoir with a statement of some strong reasons for believing that petroleum has been derived mainly from the decomposition of *marine* plants.

3. *On the oldest known British Crab* (*Protocarcinus longipes* Bell, MS.) *from the Forest Marble of Malmesbury, Wilts;* by Henry Woodward.—The author stated that three genera and twenty-five species of Brachyurous Crustacea had already been described by Prof. Reuss and H. von Meyer from the Upper White Jura of Germany; but as no limbs or abdominal segments had been met with, it was more doubtful where to place them than the species now described, which had nearly all its limbs *in situ*, and a portion of the abdomen united to it. *Protocarcinus* closely resembles the common spider-crabs—the *Maidæ* and *Leptopodidæ* living on our own coasts.—*Proc. Geol. Soc., Reader*, June 2.

4. *Memoirs of the Geological Survey of Great Britain and of the Museum of Practical Geology. The Geology of North Wales*, by A. C. Ramsay, F.R.S., Local Director of the Geological Survey of Great Britain. 382 pp. 8vo, with numerous plates, a map and sections. London, 1866. 13*s.* in boards.—In a brief introductory notice of this volume preceding the Preface, Sir Roderick Murchison says, "The Memoir upon the Geological Structure of North Wales which is now published is, I consider, the most important work which has been issued by the Geological Survey during the ten years that have elapsed since I became Director;" and we would add our testimony to the great value of the work. It treats of the earliest fossiliferous rocks of Wales, and with great fulness and exactness of description. There are 26 lithographic plates of fossils, besides sections, and a beautiful colored geological map of Wales. In the summary at page 229, Prof. Ramsay gives the following statement respecting the lowest of the Silurian beds.

"The chief object of this Memoir has now been accomplished, for I have described in detail the Cambrian and Lower Silurian rocks of Merionethshire, Caernarvonshire, and Anglesey, and in a briefer manner the Upper Silurian and other strata that lie strictly within the region I proposed to illustrate. I shall now, in conclusion, revert to some leading stratigraphical and paleontological points, by way of summary, and also partly to show the general connexion of the region described with other parts of Wales, in such a way as to explain the effect of the whole on its physical geography.

"First, then, the term Cambrian has been applied by the Geological Survey only to those strata that lie directly below the Lingula flags, and which, excepting worm-burrows, have heretofore yielded only doubtful fossils. These are the oldest strata in Wales, and are believed to be the equivalents of the Irish rocks at Bray, and of the red conglomerates and sandstones in the northwest of Scotland described by Sir Roderick Murchison. In Wales, however, we never get to their base, and whether or not they lie unconformably on gneiss, like that of the Lewes and the St. Lawrence, it is vain to speculate.

"The relation, however, of the Cambrian to the overlying strata is clear, for everywhere in Wales there seems to be conformity, and even a gradual passage from the Cambrian rocks to the Lingula flags. They are, therefore, intimately related to each other, and perhaps, except for the convenience of a great lithological distinction, they scarcely require separation by line and color.

"The Lingula flags, from 5,000 to 6,000 feet thick where thickest, contain, as at present known, about 22 species of Trilobites of the genera *Dikelocephalus* (4), *Agnostus* (5), *Olenus* (7), *Conocoryphe* (*Conocephalus*) (3), *Ellipsocephalus* (1), and *Paradoxides* (2); *Hymenocaris vermicauda*, and 3 Brachiopoda (2 *Linguleæ* and an *Orthis*), 1 Polyzoon (*Dictyonema*), and several Annelids.

"Above the true Lingula flags lie the Tremadoc slates; and Mr. Salter first proved that the fossils of these beds are mainly distinct from those of the Lingula flags below and of the Llandeilo and Bala beds above them. Thus of 11 genera of Trilobites only Dikelocephalus, Conocoryphe, Olenus, and Agnostus, are common to the Lingula flags, and the

species are entirely distinct. The remaining seven are *Angelina*, *Asaphus*, *Cheirurus*, *Ogygia*, *Ampyx*, *Psilocephalus*, and *Niobe*. The Pteropod *Theca*, and *Bellerophon*, *Conularia*, *Orthoceras*, and *Cyrtoceras*, as far as we know, first appear in the Tremadoc slates in Britain. Of the Trilobites, *Agnostus princeps* seems to be the only species common to Lingula flags and Tremadoc slate, and of a tolerably numerous list of bivalve shells *Lingulella Davisii* and *L. lepis* are the only forms that ascend from the lower horizon. It was not till after the whole of Wales had been mapped that the existence of the Tremadoc slate as a recognizable subformation was suspected, for where almost all the rocks are slaty, and where there is no visible break in conformity, minor lithological distinctions are generally of small value. All known evidence, however, tends to prove that in Wales the Tremadoc slate is a very local formation, and though searched for, none of its peculiar fossils have yet been found in Wales, except in certain spots in Merionethshire and Caernarvonshire.

"Next come the Llandeilo and Bala beds, the prodigious development of life in which had no parallel in the older British formations; and it is important to remember that the fossils of these strata are to a great extent different generically, and almost entirely specifically, from those known in the more ancient formations.

"With respect, then, to Lingula, Tremadoc, and Llandeilo and Bala beds, taking into consideration the remarkable breaks in succession not only of species but of genera, together with various physical points of great significance, I have no doubt that actual unconformity exists in this part of the series, and that there is a necessary connexion between these facts. Indeed, this unconformity, if not seen, is, as already stated, easily inferred, for while in Merionethshire the Lingula flags are from 5,000 to 6,000 feet thick, only 11 miles north, near Llanberris, their thickness is only 2,000 feet, this reduction having been produced probably by *unconformable overlap*. Close to Menai Straits, if present at all, the Lingula beds are still thinner, and in Anglesey they are absent altogether, so that the Llandeilo and Bala beds lie directly and, I believe, unconformably on Cambrian strata. To show that this is not a mere local accident, let me recall the circumstance that in Ireland and in Sutherlandshire the Lingula flags are also absent, and Llandeilo beds lie unconformably on Cambrian grits and conglomerates."

Professor Ramsay continues with a summary of his results with regard to the rest of the Silurian.

The volume closes with an appendix on the fossils (to which the plates of fossils pertain) by the able paleontologist, J. W. Salter.

5. *Notes on the formation of the Dead Sea;* by L. Lartet.—The memoir on the Dead Sea by Mr. Lartet closes with the following conclusions:

In reviewing my geological study of the basin of the Dead Sea I am led to think—

(1.) That at the end of the Eocene period, and in consequence of an upward movement (the date of the commencement of which cannot be determined) an ocean bed was protruded corresponding to the continent of Syria and Arabia Petrea.

(2.) Before this protrusion (even before the deposit of the Cretaceous rocks), disturbances had taken place in the submarine beds, and a fissure

had opened from north to south through which the feldspathic porphyries made their way, which now appear between Petra and the Dead Sea.

(3.) This fissure may have been prolonged toward the north by subsequent movements which determined the formation of the highlands of Palestine; while the fall of the eastern side of those highlands all along the line of dislocation, may have caused that narrow and lengthened depression which separates Palestine from Arabia.

(4.) The basin of the Dead Sea has thus been formed without any influence from or communication with the ocean; whence it follows that the Lake which occupies the bottom of the basin has never been anything but a reservoir for the rainfall—the saltness of which originally proceeded from the constitution of the environs of the Lake, and has greatly increased under the influence of incessant evaporation.

(5.) Toward the end of the Tertiary period, or the commencement of the Quaternary period, the water of the lake stood at more than 100 meters above its present level, and then deposited marls rich in salt and gypsum beds.

(6.) At a later period volcanic eruptions have taken place to the northeast of the basin, which produced important streams of basalt, some of which extend as far as the Jordan valley itself. Other eruptions of less importance took place directly east of the lake, of which three reached its eastern shore near the Wadys Ghuweir and Zerka Main and the south end of the plain of Zarah.

(7.) Hot and mineral springs, bituminous eruptions, similar to those which accompany and follow volcanic action, and earthquakes, which are still frequent in the district, have been the last important phenomena affecting the basin of the Dead Sea.—*Reader, April* 14.

6. *On the occurrence and geological position of Oil-bearing deposits in New South Wales;* by the Rev. W. B. Clarke.—The author first described the oil-producing schists and cannels of New South Wales as they exist at Colley creek, at the head of the Cordeaux river (Illawarra shales), at various places in the Wollondilly and Nattai valleys, at Reedy creek (Hartley cannel), Stoney creek, and elsewhere; as well as a substance resembling "Bog-butter," occurring at Bournda, and probably of very recent date. Respecting the Colley creek cannel, Mr. Clarke observed that he saw no porphyry near it, but that a seam or mass of the cannel, which here contains numerous scarcely rounded grains of quartz, was passed through in the midst of a series of layers of black, partly unctuous clay, which also contained many similar quartz grains; these grains gave to the clay a porphyritic aspect, so that by sight alone one might be led to consider them a decomposed porphyry. The chief conclusions at which the author arrived were, (1) that, with the exception of the Stoney creek cannel, all the oil-producing deposits occur in the Upper Coal-measures, and that the cannel of Stoney creek, on the river Hunter, occurs in the Lower Coal-measures, which are above the Lower Marine beds with Trilobites, below which again are numerous fossiliferous beds before the porphyry is reached; and (2) that the cannel belongs to beds in which *Glossopteris* occurs, and therefore may be a slight additional evidence to their antiquity, as it is an analogue of the "Bog Head" cannel of Scotland.—*Proc. Geol. Soc., Reader, Apr.* 21.

7. *Report on Geological and Industrial Resources of the Grand Traverse Region, or the Counties of Antrim, Grand Traverse, Benzie and Leelanaw, in the Lower Peninsula of Michigan;* by ALEXANDER WINCHELL, A.M., Professor of Geology, Zoology and Botany in the University of Michigan, and late State Geologist. 98 pp. 8vo, with a map. Ann Arbor, 1866.—The object of this pamphlet by Professor Winchell is to present to view the resources of the Grand Traverse Region—that is, the portion of Michigan on the west side of the peninsula about Grand Traverse Bay. It treats of the topography, soils, climate, timber and native useful plants and animals, geology and geological resources as regards salt, petroleum and clays, and of the farm crops, fossils, etc. Professor Winchell closes his Report with an Appendix, giving some detail with regard to the Hamilton rocks of the region, and descriptions of a considerable number of new fossil corals and mollusks. He divides the formation into (1) Pale buff massive limestones (consisting of *Pleurotomaria* beds below, as he designates them, and *Stromatopora* beds above; (2) Bituminous shales and limestones (consisting of *Bryozoa* beds below and of *Acervularia* beds above); (3) Buff vesicular magnesian limestones; (4) Chert beds. No. 1, or the lowest, graduate, on the east side of the peninsula of Michigan, into the subjacent Corniferous limestone. The total number of species of fossils observed in No. 1 is 41; in 2, 87; in 3, 8 species.

8. *New mineral localities;* by GEO. J. BRUSH.—(1.) *Diaspore.*—A year or more since Mr. W. W. Jefferis sent me some minute fragments of a hard foliated mineral found by him at Newlin, Chester Co., Pa. The substance was imbedded in emerylite, and on examination proved to be diaspore. Quite recently Mr. Jefferis has continued his explorations and has discovered the diaspore in crystals, some of which he informs me are 1 to $1\frac{1}{8}$ inches in length, $\frac{3}{8}$ to $1\frac{1}{8}$ broad, with a thickness of an eighth to a sixteenth of an inch. The only crystal I have seen is nearly honey-yellow in color, and has a high luster, especially on the plane $\breve{\imath}\imath$, or that parallel to the cleavage; it is imbedded in emerylite so that it is difficult to make out the other planes. In size the crystals surpass any that I have seen from Asia Minor or Schemnitz, and in perfection of planes they compare favorably with the beautiful crystals discovered by Prof. J. Lawrence Smith at Gumuch Dagh.

(2.) *Ouvarovite.*—Among some specimens presented to the metallurgical collection of Yale College by Mr. Clayton of San Francisco, there is a piece of chromic iron, from near New Idria, California. This specimen is covered with rhodochrome, and with a green mineral which was supposed by the collector to be emerald nickel. On examination with a magnifier the latter proves to be in druses of crystals showing the rhombic faces of the regular dodecahedron; and on blowpipe analysis the mineral was found to have the pyrognostic characters of chrome-garnet. Its association is precisely the same as that of the chrome-garnet of the Urals.

9. *On crystallized Cryolite;* by G. HAGEMANN. (Communicated for this Journal.)—Crystals of cryolite have hitherto been considered a great rarity, and the only form described is that of the simple rectangular prism. They have been much sought for with little success, and great was my surprise in finding them abundant in the cryolite of a recent cargo.

The cryolite on which the crystals were found is not of good quality, that is to say, it is much mixed with other minerals. The purer cryolite cargoes have never afforded me any traces of crystals. They occurred over the exterior of the large masses, and only in one instance have I found them lining a cavity. They were covered in all cases with a red mineral.

The most common form is a rectangular prism, either short and tabular, or long; the former sometimes 6 mm. square; the latter small, not exceeding 3 mm. in length. The prisms have occasionally a replacement of two of the angles by a triangular plane (*a*); and the base and sides are diagonally striated in a direction parallel to the sides of this plane, and in the same direction when the plane is absent. The prisms are grouped one over another, giving a stair-like surface, which is mostly covered by the red mineral alluded to. Where this mineral is absent, or has been removed, the crystals are perfectly clear and colorless. In other instances the prismatic crystals shoot up through a crust, about 1 mm. thick, covering the pure cryolite. This crust has an opal-like aspect, and is colorless, and on breaking it over the clear cryolite crystals, it can be seen that there is a passage from the underlying cryolite to the surface through this opal-like mineral.

The greatest variety in the crystalline forms were found among the crystals lining the cavity, some of which are 4 mm. through, though these are mostly broken and incomplete. The best are rectangular prisms with triangular planes on all four angles of the base, corresponding to two domes (*a*, before described, and *b*); but which of the two is the macrodome has not yet been made out. By measurement with the common goniometer the prismatic angles were found to be 90°, or at least very near this. The angle of dome *a* over the summit was found to be 70° 30′; the planes of dome *b* were too small for my measurement. On another crystal I obtained for a prism 128°; and for a dome 108° 30′; the crystal was a very small one and the measurement therefore imperfect; the calculated angles are respectively 126° 50′ and 109° 16′. Planes of other domes and pyramids exist, but they are all very small. However incomplete this examination, there can be no doubt that the crystals are trimetric in character.

10. *Paracolumbite and Corundophilite of* C. U. Shepard.—We have received from Prof. Shepard a communication in which he states that the *paracolumbite* analyzed by Pisani (see this Jour., vol. xxxvii, 1864) was not from Taunton, the original locality, but from Cumberland, Rhode Island, where a mineral occurs so closely resembling it that he himself had later pronounced it paracolumbite. Pisani's specimen, thus labeled, was from Prof. Shepard. Prof. S. claims that *true* paracolumbite has not yet been analyzed.

Prof. Shepard makes a similar claim, in this communication, with regard to *his* "*corundophilite*" of Chester, Mass., which has been analyzed by Pisani and other chemists and shown to be *clinochlore*. He says that the *true* corundophilite is that of Asheville, N. C., of which he had only 0·146 of a gram for a chemical examination.

But Prof. Shepard has published in this Journal and elsewhere that

the Chester mineral is corundophilite; he has written Dr. J. Lawrence Smith to this effect (as cited in our last number), and other persons also, including one of the editors of this Journal; and he has distributed specimens so labeled. And if he does not know his own species, it may well be inferred that it is because they have no distinctive characters. The species was founded by him on an imperfect examination with an analysis in which there was a loss of 20 per cent, and never should have been named without more knowledge.—J. D. D.

11. *Color of a diamond changed by heat.*—In May last, Prof. Fremy exhibited to the Academy of Sciences at Paris a yellowish diamond, of the size and quality that ordinarily sells for 12,000 dollars, which on being heated changes its color to rose-red; this color it retains for two or three days and then gradually resumes the original yellow. On account of this peculiarity the actual value of the diamond was stated to be three times the amount above mentioned.—*Les Mondes*, p. 85, May 10, 1866.

12. *Gieseckite a result of the alteration of Elæolite.*—The view that the mineral gieseckite, found first in Greenland and some years since at Diana in New York, is a result of the alteration of elæolite or nepheline has been held by different authors. Pisani, of Paris, has recently observed that the elæolite of Brevig, Norway, is often partly altered to a brick-red material which is true gieseckite in nature and composition. On the same specimen are found true translucent elæolite, affording only 0·33 p. c. of water on calcination, and entirely soluble in acids; along side of this, there are red spots where alteration has commenced; and beyond, the mineral is changed to a brick-red uniform material, mostly opaque, with some translucent spots of unaltered elæolite. This red material afforded 5·90 p. c. of water, and dissolved only partially in dilute nitric acid, it yielding an abundant red precipitate. On separating the insoluble portion, by treatment with dilute cold nitric acid, this afforded, on analysis, $\dot{S}i$ 46·95, $\dot{A}l$ 34·65, $\dot{F}e$ 1·86, $\dot{M}g$ 0·58, $\dot{C}a$ 0·68, $\dot{K}$ 8·71, $\dot{N}aSi$ 0·71, $\dot{H}$ 5·58=99·72, thus showing that, besides taking up water, the soda of the elæolite had been replaced almost wholly by potash. This transformation of elæolite into gieseckite is similar in many respects to that of cancrinite into bergmannite elucidated by Pisani and Sæmann in 1862 (Ann. de Ch. et Phys., lxvii). The facts prove that these apparently crystallized minerals are actually pseudomorphs. Blum has observed a specimen of bergmannite with a nucleus of elæolite, showing that bergmannite may come from the alteration of this mineral as well as cancrinite. Yet as the crystals of bergmannite are usually quite long, unlike those of nepheline, it is probable from the occurring forms that it is mostly derived from cancrinite.—*Les Mondes*, June 28, 1866, p. 368.

13. *Apophyllite made by artificial means.*—Becquerel has observed that if distilled water is made to run slowly over plates of sulphate of lime, the surface becomes chatoyant from the dissolving action of the water; and if a saturated solution of sulphate of potash be employed instead of water, a double sulphate of potash and lime is obtained, crystallized in needles; while with a solution of silicate of potash (marking 0 to 10 areometric degrees), instead of the sulphate, pearly radiated crystals are formed which are a double silicate of potash and lime, and have all the characters of apophyllite.—*Les Mondes*, July 5, p. 415.

14. *On a new variety of Spinel;* by H. St.Claire Deville.—A black spinel with low pyramids in place of the octahedral planes, and the faces rounded, has been found in the rock of Auvergne called *Lherzolite.* Most of the crystals, however, are octahedrons with rounded beveled edges and eroded faces. The crystals are 5 to 10 (rarely 20) millimeters in diameter; and though mostly black are sometimes reddish-brown. G.= 3·871 for the black; 3·868 for the reddish-brown. Analysis by Deville gave Al 59·06, Fe 10·72, Fe 13·60, Mg 17·20=100·58, whence the formula (Mg, Fe)(Al, Fe), as in true pleonaste.—*Les Mondes,* July 12, p. 458.

15. *Origin of the Diamond.*—E. B. de Chancourtois has presented the view that the diamond has been formed from hydrocarburetted emanations, as sulphur is formed from hydrosulphuretted emanations, and that its origin is thus connected with the previous existence of petroleum-bearing or bituminous schists. In the oxydation of sulphuretted hydrogen in solfataras, all the hydrogen is oxydized, but only part of the sulphur passes to the state of sulphurous acid in this humid process of combustion. So, in an analogous manner, the diamond was probably formed; that is, in the course of the humid combustion of a carburetted hydrogen in which all the hydrogen was oxydized, but only a part of the carbon was transformed into carbonic acid. This view accords with the occurrence of the diamond in arenaceous rocks or itacolumites, which are mostly metamorphic rocks of paleozoic age, and which may have once been bituminous either by original formation or by emanations from lower rocks. Mr. Chancourtois supposes that the crystal would have formed only where there were fissures for the passage of the vapors of the carburetted hydrogen, and where the process could go on with extreme slowness.—*Les Mondes,* July 19, p. 438.

[The author does not appear to connect the process of formation with that of the metamorphism to which the diamond-bearing rocks have undoubtedly been subjected, and which may have been essential to the result.]

16. *Paragenesis of Minerals.*—Reuss has a paper of great interest in the Berichte of the Vienna Academy for Jan. 7, 1863, on the associations and superpositions of the various minerals at Przibram, with reference to their origination at different periods and in various successions. The great number of metallic ores and other species in that noted mining region makes it especially instructive in this respect.

17. *Cassiterite.*—Cassiterite from Montebras in France has been found by Capt. Carron to contain two to three per cent of niobic and tantalic acids, and in some cases even five per cent, and he says that the ore may be used for obtaining these rare metals.

Cassiterite is found in the Temeschal Ranche, in Los Angeles Co., California, at several points, and with some promise of economical value. It is associated in some cases with a ferruginous porphyritic rock, or a very black compact hornblende in granite and quartzite.

Wood-tin has been found also near Boonville, Owhyhee Co., Idaho Territory, by Walter Gibson of New York.

18. *Analyses of Minerals;* (communicated by S. B. Sparkler.—(1.) *Hornblende from Birmingham, Delaware Co., Penn.* This rock is crystalline in structure, and forms a bed one to two miles long and from half

to three-fourths of a mile wide. The analysis gave: Silica 47·77, FeO 15·41, MnO ·26, Al_2O_3 7·69, CaO 13·16, MgO 15·28=99·57. It is of a very dark green color.

(2.) *Precious serpentine from East Goshen, Chester Co., Penn.* Silica 43·89, FeO 1·38, MgO 40·48, HO 13·45=99·20.

19. *On Anatase at Smithfield, R. I.;* by Rev. E. B. EDDY.—Anatase occurs at the Dexter Lime Rock, Smithfield, R. I., and is there associated with crystallized quartz, nacrite, acicular natrolite and pearl spar. The rock is dolomite. Needles of natrolite penetrate the quartz crystals in every direction, and the calcite also.—*Bost. Soc. Nat. Hist.*, x, 94.

20. *Petroleum in Russia.*—A well, six centimeters in bore, near Temrioux, in southern Russia, affords 73,000 litres of oil per day.

21. *On the Composition of the Stone implements found in Celtic monuments.*—Damour has examined the stone relics of various museums and private collections of France and Switzerland, and finds that the following minerals have been used for them: quartz (agate, jasper, flint); obsidian, fibrolite, oriental jade, or nephrite, Oceanian jade, jadeite, a rock which he calls chloromelanite, hornblende (either actinolite or black hornblende), saussurite, besides the rocks aphanite, basalt, diorite, dolerite, petrosilex. The obsidian comes from volcanoes, and is found in Europe at Cantal in France, in Hungary, near Naples, and at Milo and Santorin in the Grecian Archipelago. Fibrolite (Sillimanite) occurs in Europe in the Tyrol, in Moravia, Bavaria, and also in France in the departments of the Rhone, and the Haute-Loire. Damour concludes that some one or more of the French localities have afforded the fibrolite.—*Les Mondes*, Nov. 2, 1865.

The variety of fibrolite which seems to have been preferred by the Celts for their stone implements has been supposed, according to B. de Lom, to occur only in the vicinity of Issoire, where it is found in boulders. This author adds other localities of the variety, in the canton of Paulhaguet, and in the vicinity of Chavagnac and Ourouze, associated with mica, kyanite and red or blue corundum, and also between St. Eble and Crespignac. He mentions masses of fibrolite weighing eight to twelve kilograms. With it occurs andalusite, a mineral hitherto not found in France.—*Les Mondes*, Dec. 21, 1865.

22. *Geological Survey of Iowa.*—It gives us pleasure to state that the geological survey of Iowa has been again taken up by the State, and that $13,000 have been appropriated for two years, and Dr. C. A. White put in charge. Dr. White will we believe do well the work before him and produce Reports of great value both to the state and to science. The terms of his appointment impose on him and his assistants the duty of giving "the people of the State the greatest amount of practical information in relation to its resources;" and require the state geologist to publish popular articles in the newspapers of the state; also to deposit in the State University all type specimens, and in the Agricultural College all those illustrating economic geology.

23. *New Paleozoic Crustacea and Cirriped.*—Mr. HENRY WOODWARD has described in the Journal of the Geological Society of London for November, 1865, two new species of Devonian Crustaceans of the Eurypterus family, and genus Stylonurus, and a third, between Limulus and

Eurypterus in character, obtained from the Lower Ludlow beds, for which he proposes the generic name *Hemiaspis*. He also shows that the *Chiton Wrightianus* of de Koninck, from the Wenlock beds, is not a Chiton but a Cirriped, and he names the genus *Turrilepas*. This is the only species of Paleozoic Cirriped yet discovered, and has therefore great geological interest. The remains of these species are figured on two plates.

24. *The Geological Magazine, or Monthly Journal of Geology* (with which is incorporated "The Geologist"); edited by HENRY WOODWARD, F.G.S., F.Z.S., Professor JOHN MORRIS, F.G.S., &c., and ROBERT ETHERIDGE, F.R.S.E., F.G.S., etc.—The third volume of this Magazine of Geology, published by Messrs. Trübner & Co., London, commenced with January of the current year. It is issued in monthly numbers of 48 pages each, illustrated by plates and woodcuts. It has an able editorial corps, and numbers many of the first geologists of England among its contributors; and the interest and value of its papers entitle it to a large American circulation. A plate in No. 3 (March) gives excellent views of the wings of a Libellula from the Stonesfield beds, illustrating a paper by Prof. John Phillips; two others, representations of the jaws and teeth of a new Sauroid fish from the Kimmeridge Clay, described in an article by Prof. Owen; and another, sections of an ancient beach and submerged forest near Calais. In No. 5, Prof. Owen has an illustrated paper on a small mammal from the Upper Oolite of Purbeck which he calls *Stylodon pusillus*, the specimen of which is part of the lower jaw with the teeth, yet is insufficient for a decision as to whether it is Marsupial or not. Besides original papers, this excellent magazine gives reports of the geological papers in the Proceedings of different societies, and reviews of new works. The price per number is 1*s*. 6*d*.

III. BOTANY AND ZOOLOGY.

1. WILLIAM HENRY HARVEY, whose lamented death was announced in the last number of this Journal (p. 129), was born at Summerville, near Limerick, Ireland, on the 5th of February, 1811. His father, Joseph M. Harvey, was a highly respected merchant in that city, and a member of the Society of Friends. William Henry was, we believe, the youngest of several children. He received a good education at Ballitore School, —an institution of the Friends, and on leaving it was engaged for a time in his father's counting-room, devoting, however, all his spare time to Natural History, his favorite pursuit even from boyhood. He made considerable attainments in Entomology and Conchology, and in Botany he early turned his attention to Mosses and *Algæ*. To the study of the latter, in which he became preëminent, he was attracted from the first by the opportunities which he enjoyed on the productive western coast of Ireland, the family usually spending a good part of the summer at the sea-side, mostly on the bold and picturesque shore of Clare. As the late Sir Wm. Hooker's bent for botany was fixed by his accidental discovery of a rare moss, which he took to Sir J. E. Smith, so in turn was Harvey's, by his discovery of two new habitats of another rare moss, the *Hookeria lætevirens*, which led to a correspondence with Hooker, and to a life-long mutual attachment of these most excellent men. Encouraged

by his illustrious friend and patron, Harvey sought some position in which he might devote himself to science; and it would appear was selected by Mr. Spring Rice (the late Lord Monteagle), for the post of Colonial Treasurer at the Cape of Good Hope; that by some accident the appointment was made out in the name of an elder brother, and an inopportune change of Ministry, frustrated all attempts at rectification. There was no other way but for the brother to accept the undesigned appointment, and take the young botanist with him to the Cape as his assistant. This was done, and the brothers sailed for that colony in the year 1835. But the health of the elder brother suddenly and hopelessly failed within a year, and he died in 1836 on the passage home. William Harvey's appointment to succeed his brother had been sent to the Cape while he was on his homeward voyage: he immediately returned to his post, and fulfilled its duties for three years, devoting his mornings to collecting and his nights to botanical investigation, with such assiduity that his health also gave way, and he was compelled to return home in 1839. The summer of the next year found him reëstablished, and on his way to the Cape for the third time. But he could not long endure the sultry climate and the intense application; with broken health he came back in 1841 and gave up the appointment.

After two years of prostration and seclusion he was well again; and, in 1844, on the death of Dr. Coulter, he was appointed Keeper of the Herbarium of Trinity College, Dublin. The most important portion of the herbarium then consisted of the collections, yet unassorted, made by Coulter in northwestern Mexico and California. Harvey generously added his own large collections, for which he was allowed fifty pounds a year, in addition to a slender salary, and he proceeded to build up the herbarium into a first-class establishment. The professorship of Botany in the College, which was pretty well endowed, fell vacant about this time; and the College authorities, wishing to elect Harvey to the chair and so to combine the two offices, conferred upon him the necessary degree of M. D. But it was contended that an honorary degree did not meet the requirements, and so Dr. Allman, the present distinguished professor of Natural History at Edinburgh, carried the election.

Except for the slenderness of his salary, Dr. Harvey was now well placed for scientific work, the object to which he wished to devote his life; and he entered upon and pursued his distinguished career henceforth with an entire and well-directed energy that never flagged until he was prostrated by mortal disease.

He had already published, at the Cape in 1838, his *Genera of South African Plants*, hastily prepared, solely for local use, but no unworthy beginning of his work in Phænogamous Botany; and in his favorite department of the science he had brought out, in 1841, his *Manual of British Algæ*, which he re-edited in 1849. He now commenced the first of the series of his greater works, illustrated by his facile pencil,—for he drew admirably. The first (monthly) part of his excellent and beautiful *Phycologia Britannica, a History of British Seaweeds*, containing colored figures of all the species inhabiting the shores of the British Islands, appeared in January, 1846; and the undertaking was completed in 1851, in three (or four) volumes, with 360 plates, all drawn on stone

by his own hand. A similar but less extended work, the *Nereis Australis, or Algæ of the Southern Ocean*, which was begun in 1847, was carried only to 50 plates, of selected and beautiful species.

In 1848, Dr. Harvey succeeded Dr. Litton as Professor of Botany in the Royal Dublin Society, to which belonged the Botanic Garden at Glasnevin; this required him to deliver short courses of lectures annually in Dublin or in some other Irish town, and provided a welcome addition to his income.

In 1848, at the request of his friend Van Voorst, the publisher, he wrote his charming little volume, *The Sea-side Book*, the unsurpassed model of that class of popular scientific books; it was published in 1849, and has passed through several editions. In July of that year, having arranged a visit to this country, and having been invited to deliver a course of lectures before the Lowell Institute, he took steamer for Halifax and Boston, passed the summer and autumn in exploring the shores of the Northern States, and in the society of his friends and relatives: for the late Mr. Jacob Harvey, still well and pleasantly remembered in New York, who married the daughter of Dr. Hosack, was his elder brother. In the autumn he gave an admirable course of lectures upon Cryptogamic Botany before the Lowell Institute, in Boston, and afterwards a shorter course at the Smithsonian Institution at Washington. He then travelled in the Southern Atlantic States, continuing the exploration of of our *Algæ* down to Florida and the Keys; and in May, 1850, he returned to Ireland.[1] Under the wise and liberal arrangements made by Prof. Henry, in behalf of the Smithsonian Institution, and with his own large collections augmented by the contributions which every student or lover of *Algæ* was glad to place in such worthy hands, Prof. Harvey now prepared his *Nereis Boreali-Americana, or Contributions to a History of the Marine Algæ of North America.* The work is a systematic account of all the known marine Algæ of North America, but with figures only of the leading species. It was issued in three parts; the first part, the *Melanospermeæ*, in 1852 in the third volume of the Smithsonian Contributions to Knowledge; the second, the *Rhodospermeæ*, in the fifth volume; and the third, or *Chlorospermeæ*, in the tenth volume of the series, published in 1858; and the three parts, collected for separate issue, compose a thick imperial quarto volume, of 550 pages of letter-press and fifty plates. The work remains the principal if not the only guide to the American student of *Algæ*, and one of the most popular as well as useful of the very various contributions to knowledge which the well-managed bequest of Smithson has given to the world.

Before the last part of the *Nereis Boreali-Americana* was published, Prof. Harvey had sought a wider field of scientific labor and observation. Obtaining a long leave of absence, and some assistance from the University in addition to the continuance of his salary, he left England in August, 1853, by the overland route for Australia, stopping at Aden and Ceylon to collect: he visited the east, south and west coasts of Australia, as well as Tasmania. Taking advantage of a missionary ship, which was

[1] A notice of Dr. Harvey in the Athenæum states, quite erroneously, that "he also at this time made a tour around the shores of the Pacific, visiting Oregon and California."

to cruise among the South Sea Islands, and which offered him unexpected facilities, he visited the Fiji, Navigators' and Friendly Islands, touching also at New Zealand. Returning to Sydney, he sailed to Valparaiso, which he reached much prostrated through over-exertion in a warm climate; and when recuperated he returned home by way of the Isthmus, arriving in October, 1856. The algological collections of these three laborious years, or the Australian portion of them, formed the subject of Prof. Harvey's third great illustrated work, and one of the most exquisite of the kind, the *Phycologia Australica*, the serial publication of which began in 1858 and was concluded in 1863, in five imperial octavo volumes, each of 60 colored plates. All but the last century of plates were put upon stone by the author.

Upon Dr. Harvey's return, in 1856, from his long expedition, he found the chair of Botany in the University of Dublin vacated by the appointment of Dr. Allman to that of Natural History in the University of Edinburgh; and he was at once preferred to the position which he had sought when younger and freer, and which he now occupied till his death. The exhausting duties of this chair, and of that which he still held in the Royal Dublin Society, undiminished by the transference to the Government Museum of Irish Industry, did not prevent Prof. Harvey from entering with unabated ardor upon an undertaking of greater magnitude than any preceding one. This was the *Flora Capensis*, a full systematic account of all the plants of the Cape Colony and the adjacent provinces of Caffraria and Natal,—in which he was associated with Dr. Sonder of Hamburgh. Three thick octavo volumes of this work have appeared, the last in 1865, including the *Compositæ*. Along with this Dr. Harvey —learning for the purpose another form of lithographic drawing,— brought out, between the years 1859 and 1864, two volumes of his *Thesaurus Capensis, or Illustrations of the South African Flora*, comprising 200 plates of interesting phænogamous plants. A complete list of his publications would include several contributions to scientific periodicals, mainly to Hooker's Journal of Botany, and a few miscellaneous writings.

In April, 1861, Dr. Harvey married Miss Phelps of Limerick. If not robust, he was apparently in good health, in the full maturity of his powers, and it was hoped only at the noonday of his allotted course of usefulness. But ere the lecture-season of that summer was over, an attack of hæmorrhage from the lungs gave notice of a serious pulmonary disease. Yet he seemed to recover from this almost completely: he resumed his stated work, and gave his lectures as usual in 1863, and also in the spring of the following year, but with some difficulty. The winter and spring of 1864–5 were spent in the south of France, with only transient benefit. Returning to his home and his herbarium he worked on still at the Cape Flora, with cheerful spirit but feeble hands, until he could work no longer. Last spring he sought in Devonshire a milder air, and found a peaceful rest. "On Tuesday, the 15th of May, 1866, at the age of 55 years, he quietly breathed his last, at the residence of Lady Hooker, the widow of his long-attached friend Sir William J. Hooker, surrounded by kind and anxious relatives and friends, and was buried in the cemetery at Torquay on Saturday the 19th of May."

Dr. Harvey was one of the few botanists of our day who excelled both in phænogamic and cryptogamic botany. In Algology, his favorite branch, probably he has left no superior; in systematic botany generally he had now an eminent position. He was a keen observer and a capital describer. He investigated accurately, worked readily and easily with microscope, pencil and pen, wrote perspicuously, and where the subject permitted, with captivating grace; affording, in his lighter productions mere glimpses of the warm and poetical imagination, delicate humor, refined feeling, and sincere goodness which were charmingly revealed in intimate intercourse and correspondence, and which won the admiration and the love of all who knew him well. Handsome in person, gentle and fascinating in manners, genial and warm-hearted but of very retiring disposition, simple in his tastes and unaffectedly devout, it is not surprising that he attracted friends wherever he went, so that his death will be sensibly felt on every continent and in the islands of the sea. A. G.

2. Dr. Robert Kaye Greville, the distinguished predecessor of Dr. Harvey in British Algology, and for many years a prominent investigator and illustrator of other branches of the Lower Cryptogama, the collaborator of Sir Wm. Hooker in the *Icones Filicum*—died at his residence in Edinburgh on the 4th of June last, in the 72d year of his age. His interest in various departments of Natural History was kept up to the last, but of late years he turned his attention mainly to the *Diatomaceæ*. More than twenty years ago, owing to some change in his formerly independent circumstances, he felt it necessary to turn his artistic talents to account, and he took to marine and landscape painting as a profession. Almost throughout his life he was active in various social reforms, he took a prominent part in the agitation against slavery in the British Colonies; and he leaves a highly honored name as a Christian philanthropist as well as Botanist. A. G.

3. *Dr. C. Fournier on Cruciferæ, and Sisymbrium in particular.*—A quarto memoir of 154 pages and two plates, comprising a full monograph of *Sisymbrium* (166 species), prepared by various anatomical researches, and some general views on the arrangement of *Cruciferæ*, forms an unusually imposing thesis, presented to the Faculty of Sciences of Paris, for the doctorate in science, M. Fournier moreover having already that in medicine. In the histological portion (which we merely glance at), the author points out a beautiful radiate striation of the superficial cells of the petals in *Sisymbrium Alliaria* and other species, as also in *Alyssum saxatile;* and the structure of the lining of the cells of the ovary and of the septum is particularly studied. He adopts Lestibudois' view that the septum originates from the tissue of the placentæ, and not from the re-entering borders of the carpels, as DeCandolle supposed.

Touching lightly upon the nature of species, he inclines towards the views of Jordan rather than those of Naudin, believes that the species of a genus are all really circumscribed and definable, and that when two forms differ morphologically even by slight though constant characters, these may often be fortified by equally constant histological differences, the coincidence demonstrating the distinctness of the two types. He develops and makes good use of a principle brought out prominently by M. Duval-Jouve, which he calls "the principle of the parallel variation

of congeneric types." It amounts to this quite familiar idea, that the common causes of those ordinary variations which depend upon external influences are dryness on the one hand and moisture on the other. He supposes that almost all varieties are reducible to these two sorts, dryness causing an arrest, and moisture an exuberance of vegetative development, and so the varieties fall mainly under the two types, and take the convenient names, of *xerophilum* and *hygrophilum*. But species also vary more gravely and unaccountably, and in characters which are commonly of specific rank, e. g. in the direction of the pedicels and of the pods, in the length of both and in that of the style, etc. In his exhaustive treatment of a polymorphous European *Sisymbrium*, in which his study of an abundance of forms has led to the reduction of very many nominal species, we gather analogies which, if as favorably placed for the study, our author might probably have advantageously applied to an American group; which we may again refer to.

As to the general arrangement of Cruciferæ, Dr. Fournier moves with decided steps in the direction to which botanists have been tending ever since DeCandolle's re-organization of the family came to be tested. The conclusion is, that the characters drawn from the embryo, at least from the form and position of the cotyledons and radicle, have been overrated; that the primary groups may be advantageously reduced in number (he adds a sketch of a classification down to the tribes which may favorably compare with that of Bentham and Hooker); and that the genera admitted are still too numerous. In the present essay he extends *Sisymbrium* beyond Bentham and Hooker's limits, to comprise *Braya*, *Halimolobus*, and *Eutrema*. Upon the monograph of *Sisymbrium* we wish to comment upon three or four American species:—

S. Niagarense is founded on a specimen which was collected by Count Castelneau on the banks of Niagara river, and is said to differ from that common introduced weed, *S. officinale* "*fere unico pedunculo filiformi*." The fruiting pedicels are 2 to 2½ lines long and slender, instead of only 1 to 1½ lines long and thickened. Our local botanists should look after it; but we suppose it is a mere *lusus* of *S. officinale*, which is not indigenous to America.

S. teres. The obscure *Candamine teres* of Michaux was doubtfully referred to *Sisymbrium* in Torrey and Gray's Flora. Dr. Fournier has suppressed the interrogation; but his account of the plant is made up from what was already published, with a considerable error in translating. "Siliques erect, one-third of an inch in length," is rendered "*Siliquæ erectæ, vix tertiam partem lineæ longæ*," a very short silique indeed, to be, as Michaux states "*breviuscule linearibus*." As, in the Flora above referred to, the cotyledons are said to be "distinctly incumbent," we are bound to direct attention to a remark in the first edition of Gray's Manual of Botany, p. 34, where it is stated that "the plant appears clearly to be *Nasturtium tanacetifolium* or *N. lyratum* of the Southern States (*cotyledons accumbent!*), which leads me to suspect a mistake in the record of the locality." So far as the portion of an authentic specimen (given to the writer in 1839 by the late Achille Richard) allows the comparison, it accords fully with a plant collected below New Orleans by Dr. Riddell and by Berlandier (his No. 1940). Yet it may be a starveling *N. palustre*, and in that case really gathered at Lake Champlain.

S. Sophia and its relatives. If Dr. Hooker has taken one extreme view in suggesting the union of numerous American forms with this old-world species, Dr. Fournier has certainly gone to the other, and has introduced some new species upon insufficient grounds or wrong conjectures. A review of the materials before us leads to the following remarks upon the North American forms :—

S. canescens Nutt., is our only species with decidedly two-ranked seeds, these being much narrower than the partition. The pods vary from short-oblong or slightly clavate-oblong to oblong-linear and are shorter than their horizontal or sometimes ascending pedicels. *S. brachycarpum* Rich. is a short-fruited form of this, at least in part. *S. Cumingianum* Fisch., is the South American representative, with pods longer than the pedicel; but some of Gillies' specimens might well justify Hooker and Arnott's reference of them to *S. canescens.*

S. incisum Engelm., to which belongs *S. longepedicellatum* (excl. syn.), and probably *S. streptocarpum* of Fournier, is the plant which most approaches *S. Sophia*, having slender pods with one-ranked seeds. We should think it Richardson's *S. brachycarpum* from his phrase "*siliquis linearibus*," and the "*Facies S. Sophiæ, at facile distinguitur siliquis duplo brevioribus*," but the added remarks "*4 lineas longa, lineam lata*," and "*pedicello patenti brevior*," indicate *S. canescens.* The pods are only from 4 to 6 or rarely 7 or 8 lines long, and more or less assurgent on widely spreading or horizontal pedicels which are sometimes of their own length or even longer, but are usually considerably shorter. It differs from *S. Sophia*, then, in the shorter and spreading pods. But Mexican specimens, such as Coulter's 683 and Gregg's 408 (referable we suppose to Fournier's *S. Galeottianum*), with their decidedly ascending pedicels and pods, bridge the interval.

S. Hartwegianum Fournier (the Hartwegian specimen of which we unaccountably lack, but we have the Saskatchawan plant of Bourgeau) is known by its short erect-appressed pedicels and pods, the latter short-linear or somewhat fusiform, and 4 to 5 lines long, crowded with seeds in a single rank, or obscurely two-ranked, but each when well developed as wide as the partition, the pedicels a line and a half long. This is Hall's No. 40, from the Rocky Mountains, and was also collected by Burke and by Fremont; doubtless also by Nuttall, (var. brevipes) but we have not his specimen. We should account it distinct, but must admit that a part of Hall's No. 40, and a plant of the same region in Henry Engelmann's collection, effect the transition to *S. incisum.*

S. Sophia L., marked by its elongated linear pods (mostly an inch long), erect or assurgent on ascending pedicels, and one-ranked seeds, belongs chiefly to the Old World, but is naturalized in Lower Canada. The phrase in Torrey and Gray's Flora, "pedicels 4 times the length of the calyx," which shows in Dr. Fournier's opinion that the Quebec specimens cannot belong (as they surely do) to *S. Sophia* (see Bull. Soc. Bot. France, xi, p. 360) was simply copied from DeCandolle's character of that species! We may add that neither dried specimens nor the published figures enable us to see the application of Fournier's character, "*pedicelli erecto-appressi*," or "*jamais son péduncle ne cesse d'être appliqué contre l'axe.*" *S. sophioides* of Fischer, according to Dr. Hooker, is an

arctic and seemingly abnormal form of *S. Sophia*, with permanently abbreviated racemes. His father's description of the pods, as 2 inches or more in length, is indirectly contradicted by his figure of the plant "of the natural size," in which the pods scarcely exceed an inch. This is about the length in an arctic specimen, collected by Dr. Seemann, in which the axis of the racemes is considerably lengthened. In it and in some Himalayan specimens of *S. Sophia*, the seeds appear to be oblong instead of short-oval, as they are in the Canadian (introduced) and European plant.

While, therefore, with our present materials, we should acknowledge these four species in North America, we could not affirm that they are strictly circumscribed and definable; and it is quite likely that at Kew we might deem the discrimination hopeless. A. G.

4. *The Genera of Plants;* by RICHARD ANTHONY SALISBURY, F.R.S., etc.—*A Fragment containing part of Liriogamæ.* London: Van Voorst. 1866, pp. 143, 8vo.—The name of Salisbury is familiar, as appended to a good many generic names; but the present generation know little of a botanist who was prominent in the first quarter of the century, who had very much to do in bringing forward the natural system in Britain, but who was too peculiar, too much of an Ishmaelite—hating and being hated—to have justice done him in his own day. Our own impressions of him were derived from the portrait sketched by DeCandolle, in his *Souvenirs*. Dr. J. E. Gray, one of the few surviving naturalists who knew him, has here printed one of his manuscripts, and in a preface has given some account of the man. His name, it appears, was Markham, and he was born in 1761. In 1785 he took the name of Salisbury and ten thousand pounds in the three per-cents from a very old maiden lady of that name, who made him her heir. He died in London in March, 1829. In his turn he proposed to more than one botanist to leave to him his library and his fortune—to DeCandolle, as he tells us for one—on the condition of assuming the name of Salisbury. He actually left a part of his property and his MSS. to the late Dr. Burchell. Since Dr. Burchell's death, two years ago, his sister made over Salisbury's MSS. to Dr. Gray, who intends to present them, when put in order, to the British Museum. He has "here printed one fragment of the '*Genera Plantarum*,' exactly as it was left by the author, for the purpose of showing the kind of work that he intended to produce." In order to secure an exact reproduction of the MSS. the editor read no proof-sheets. He finds, as might have been expected, "that there are a few bitter personal expressions, which it might perhaps have been desirable to expunge." But if the MSS. was to be printed at all, it strikes us that there was no other course to pursue, and no harm can now come of it. *Pleurothallæ* is Salisbury's name for *Monocotyledones*, and *Liriogamæ*, for the petaloideous or non-glumaceous *Monocotyledones*. The fragment relates principally to Liliaceous and Amaryllideous genera, and is very curious and interesting. It must of course be insisted that future *Genera Plantarum* are not to be burdened with the synonymous names which here first see the light,—e. g. *Xeniatrum* for Rafinesque's *Clintonia*, *Neolexis* for *Smilacina*, &c. The interest of this opuscula is solely historical and critical. We could wish that the able editor had put upon record a general account of what Salisbury did for botany. A. G.

5. *Handbook of British Water-weeds or Algæ*, by Dr. JOHN EDWARD GRAY, F.R.S., &c.—*The Diatomaceæ*, by W. CARUTHERS, F.L.S., &c.—London: Hardwicke, 1864. pp. 123, 18mo.—An excellent little manual, and one which may be very useful and convenient in this country also (so many of the *Algæ* being identical), containing as it does the Fresh Water species, but these only in an arranged list, with references to the leading figures. The *Desmideæ* and *Diatomaceæ* add much to its value.

A. G.

6. *Scolopendrium officinarum in Western New York: probable determination of the original locality of Pursh;* by J. A. PAINE, JR.—At the request of Dr. Gray, a trip to the hills of south Herkimer County for rare orchids, was lately extended to Onondaga County for the identification, if possible, of the habitat of this fern, so rare with us, which Pursh discovered and recorded. The ravine of Chittenango creek is too far east by twenty miles or more to be referred to his remark. Jamesville, therefore, was visited to find out how far this new station is from "Onondaga," and if near or upon lands which ever were "plantations of J. Geddis, Esq." At once it was seen that this locality—detected last March by Mr. Lewis Foote, as announced in the May number of this Journal—though not far from Onondaga Hill, is far and nearly in an opposite direction from the residence and possessions of the late James Geddes, which are directly west of Syracuse. Mr. Foote having particularly described his station as in a rocky ravine, half a mile below the village, two hundred feet east of the railroad, &c., it was taken for granted that the place thus designated was in one of three or four points where the bed of Butternut creek narrows into rocky gorges, or at the entrance of a tributary stream, so a second observation appeared unnecessary. Attention however, was directed to two or three interesting localities known as "pit-hole lakes," deep depressions in the surface, walled round on all sides but one with rock at least one hundred feet high, a quarter of a mile across from side to side, usually having a small pond in the center with no visible outlet, localities of which no satisfactory explanation has been given, but greatly resembling whirlpools, as the one in the Niagara river. On the shaded talus of the nearest of these, "Little lake," about one mile west of the town, *Scolopendrium* was detected in limited quantity, with *Camptosorus rhizophyllus*. "Green pond" and "White lake" occur near together, two miles east of Jamesville, at the base of a remarkable outcrop of the limestone range, from one to two hundred feet high and four or five miles long, the former similar in character to Little lake, and lying far within the irregular line of the cliff, like a bay along its coast. These "highlands," before they were cleared and burned over, formed the very kind of locality where our rare fern delights to dwell, possessing all the conditions of loose limestones, rich mould, moisture and shade; and no doubt, their high rocky steeps formerly abounded with it. This presumption is confirmed by the fact that on a particular part of the range, where the fire and clearing ceased and the undisturbed forest began, on the talus of a low ledge, just there was *Scolopendrium* found growing in its greatest luxuriance and scattered along the bank for a fourth of a mile or so, as far as covered by woods. Directions to other like places by a gentleman

in the village who recognized the plant, indicate that it may not be infrequent throughout the town.

Onondaga valley affords frequent outbreaks of the same limestone rock along its sides and in gorges of streams descending to the creek, where this fern may grow.

Hon. George Geddes, son of the J. Geddes, Esq. referred to by Pursh, was then appealed to for information in general respecting this fern or its earliest station, and he readily cleared up the whole mystery. The place where it was discovered, he said, was nearly five miles west of Syracuse and half a mile south of his father's house; on the single point of its being on his father's farm Pursh must have erred; but it was near by, along a high ledge and about a celebrated sulphur spring. Mr. Geddes very kindly extended the hospitalities of the same mansion in which Pursh made his stay while exploring in this region, and accompanied the writer to a locality called Split-rock, half a mile south of Fairmount, the residence of Mr. Geddes, who confidently believes this to be the place where *Hart's-tongue* was discovered and formerly flourished. He recollects perfectly well how, when a boy, the existence of the fern having been doubted, his father charged him to search carefully for it in his hunting excursions, and directed him specially to this locality. Split-rock is another development of the limestone formation, probably one hundred and fifty feet high and over half a mile long, semicircular, with a brook at its base on whose bank is the sulphur spring. Its lofty and long rocky slope beneath the cliff, once a most favorable station for *Scolopendrium*, was long since cleared, dried up, and trodden over by cattle. *Walking-ferns* still linger, and even abound where there is any shade, but it is to be feared that all *Hart's-tongues* have perished.

In Madison county this plant may be looked for among the upper branches of Cowaselon creek east of the Chittenango valley, which pass through ravines and over falls; and around a number of pit-hole lakes westward. The station below Chittenango falls, brought to light about the year 1830 by William Cooper, Esq., which for so long time has been regarded as the only locality of this plant on our continent, therefore must have been unknown to both Pursh and Nuttall. The record of the latter, "*S. officinarum, v. v.* In the western parts of the state of New York, in the crevices of calcareous rocks, beneath the shade of the Hemlock Spruce (*Abies canadensis*), and accompanying the *Taxus canadensis* or American Yew," probably is merely a confirmation of the habitat of Pursh. His statement, "near Canandaigua, at Geddis's Farm, in a shady wood, with *Taxus canadensis*," as reported by Dr. Pickering to Dr. Torrey to have accompanied specimens in the herbarium of the Academy of Natural Science in Philadelphia, most likely was an error for near Onondaga, &c., easily made from similarity in the names, or from the indefinite extent covered by the former name at that time, 1806–1818. However, no such statement now exists in the herbarium at Philadelphia with Nuttall's specimens; and for the identity of his with the habitat of Pursh as above ascertained, we have "Geddis's farm," with both *Abies canadensis* and *Taxus canadensis* remaining in abundance near by.

The connection of *Scolopendrium* with Lake Simcoe, Canada West, as given in this Journal and repeated in a Catalogue of Oneida County Plants, has been a mistake for Owen Sound on the Georgian Bay. Here

it was discovered in 1857 by Professor William Hincks, growing plentifully on the rocks around the falls of a stream emptying into the Sound; since then it has also been observed by others in adjacent localities.

Geologically, this fern is confined to the limestones, and may be searched for wherever the Helderberg, Niagara and Trenton groups afford favorable stations.

Cambridge, June 15, 1866.

7. *Icones, Histiologicæ, oder Atlas der vergleichenden Gewebelehre; zweite Abtheilung. Der feinere Bau der höhereren Thiere.* Erstes Heft. Die Bindesubstanz der Cœlenteraten, mit x Tafeln und 13 Holzschnitten; by A. Kölliker. Leipzig, 1865.—This work is an elaborate essay upon the microscopic structure of Polyps and Acalephs, but more especially, upon the hard parts of the Halcyonoid Polyps. It is well illustrated by numerous beautifully executed plates and cuts, giving the details of the structure of sections of the axes of Gorgoniæ, &c.; the peculiar forms and structure of the calcareous spicula, observed in all Alcyonaria; the integuments of various Hydroids, etc. As a work illustrating the histology of these classes of animals it is invaluable and far beyond any preceding work on the same subject.

On pages 131 to 142 the author has given a Synopsis of the Classification of the Halcyonoid Polyps so far as known to him, and has introduced many new species and several new genera, with many important changes, based mainly upon the microscopic structure of the hard parts.

The genus *Primnoa* is maintained with its original limits, the subdivisions of Dr. J. E. Gray not being recognized. The genus *Muricea* is restricted to those forms like *M. spicifera* Lamx., while for the group having *M. placomus* Ehr. as its type, the new genus *Paramuricea* is established, with four species. The new genus *Echinogorgia*, allied to the two last mentioned, has the *Gorgonia sassapo* Esper, as its type, and includes four other species, all figured by Esper. This genus seems to be equivalent to my *Lissogorgia*,[1] proposed Feb. 1864, but which was not actually published until Oct. 1865, so that the former appears to have the precedence. The two species referred to (*E. flabellum* and *E. flexuosa* nov.) do not appear to be identical with either of those mentioned in this work. The genus *Plexaura* is restricted to forms like *P. flexuosa* Lamx., and for another group having *P. dichotoma* as its type, the genus *Plexaurella* has been established, with six species. The limits of the genus *Gorgonia* have been enlarged by the reunion of *Pterogorgia*, *Leptogorgia*, *Lophogorgia*, *Xiphigorgia*, *Rhipidogorgia*, *Hymenogorgia*, *Phyllogorgia*, *Phycogorgia*, &c., with the typical *Gorgoniæ* of M. Edw. and Haime. The genus *Erythropodium* is proposed for *Xenia carybæorum* Duch. et Mich., and this, with *Sympodium*, is placed in the family *Briariaceæ*.

The author is, however, certainly at fault in uniting *Gorgonia suberosa* Ellis, *Alcyonium plexaureum* Lamx., and *A. asbestinum* Pallas, into one species (*Br. suberosum* Dana), for, as I have previously shown,[2] they represent three very distinct species and two genera. But *Briareum palmachristi* Duch. et Mich. is probably identical with *B. asbestinum*. This is

[1] Proceedings Boston Soc. Nat. History, x, 22, and Proc. Essex Institute, iv, 187.

[2] Bulletin of the Museum of Comp. Zoölogy, No. 3, p. 39, and Revision Polyps E. Coast U. S., p. 10.

doubtless owing to the lack of specimens of *G. suberosa* Ellis. For the *Gorgonia suberosa* Esper, the new genus *Sclerogorgia* is established, which, as indicated, appears to be the same as *Suberogorgia* Gray. The *G. patula* Ellis and *G. verriculata* Esper are referred here, and a subfamily, *Sclerogorgiaceæ*, is instituted for the group.

The work is of peculiar value in systematic zoölogy because the author has had, for examination and illustration, the original specimens of Esper, as well as of Duchassaing and other writers, thus restoring to the science many species that have long been regarded as doubtful, or altogether neglected by many recent authors, although very well described and figured by Esper. A. E. V.

8. *The Anatomy and Physiology of the Vorticellidian Parasite* (*Trichodina pediculus* Ehr.) *of Hydra;* by H. James-Clark. From the Memoirs of the Boston Society of Natural History, with a lithographic plate. February, 1866.—In this paper the author has given an elaborate and very complete account of the anatomy, physiology, and habits of one of the most singular forms of ciliated Infusoria, and has thereby afforded the means of gaining a clearer knowledge of the general structure and classification of the entire group of Protozoa.

This parasite is stated to be the form mistaken by Prof. Agassiz for the free *Medusæ* of *Hydra*, of which the discovery was announced in the Proceedings of the Boston Soc. of Natural History, Nov. 1850, p. 354. The investigations upon the structure of this species has an additional interest on account of the views held by some authors that the *Vorticellidæ* are closely allied to the *Bryozoa*.

According to Prof. Clark, all previous figures of this species represent it in an abnormal or diseased state. "The *peristome* is not a closed circle as in Vorticellidæ proper, but follows the spiral course of the vibratory crown, and vanishes near the aperture of the vestibule. The *vibratory crown* consists of a single row of vibrating cilia, which winds along the margin of the spiral, dexiotropic peristome, just at the edge of the cupuliform disk, and descends thence to the left of the vestibular aperture, and entering it, plunges to the bottom of the vestibule, in an unbroken line. Neither Trichodina, nor any of the Vorticellidæ, possess a vestibular lash or bristle, and the latter is an optical illusion. The posterior truncate end of the body is margined by a well-defined annular *velum*, immediately behind which, and arising from the same basis, is a complete circle of vibrating cilia." * * * "The vestibule opens near and posterior to the cilia-crowned margin of the sunken cupuliform disk. The anus opens into the vestibule a short distance from its mouth, and on the right side. The contractile vesicle is a simple cavity, which performs its systole once in fifteen seconds." A. E. V.

9. *The Arctic Annelids: Nordiska Hafs-Annulater, af* A. J. Malmgren. Oefversigt af K. Vet.-Akad. Förhandlingar, 1865, Nos. 1, 2, 3.) Stockholm, 1865–66. 8vo, pp. 135, with 19 plates.—This most useful work on the higher Annelids of the Arctic seas, embracing collections made at Spitzbergen, Greenland, Iceland and Finmark, will prove invaluable in studying the species of our own coast. It is a monograph of the arctic Dorsibranchiate and Tubicolous Annelids, with numerous synoptical tables of the genera, and detailed descriptions of all the species and genera, whether new or previously edited. Three new

families, the *Ampharetea*, *Sabellacea*, and *Eriographidea*, are characterized. 52 new genera and 52 new species are described. The great number of genera proposed, at least give evidence of a careful study of these difficult forms, though sometimes the generic characters seem too slight. As most of these genera, with the same or closely allied species, are likely to occur on our own coast, the work will form, with the older work of Oersted, quite a complete manual of our Annelids. The descriptions are in Latin, and the work is thus rendered accessible to all. The illustrations are very full, nearly every species being figured, and though sometimes stiff, are in the main excellent. A. S. P.

10. *On Collections of Bones of recent Rattlesnakes in fissures in limestone near Howe's Cave;* by WM. A. ANTHONY. (From a letter to one of the Editors, dated Franklin, Del. Co., N. Y., Aug. 15, 1866.)—On my way to Albany last week, in company with Prof. Orton of Antioch college, I stopped at "Howe's Cave," and there learned that at a stone quarry, about a quarter of a mile distant, the workmen had struck into a fissure filled with the bones of rattlesnakes. We visited the spot, found the bones very abundant, and collected a few specimens. We thought the subject of sufficient interest to warrant further investigation, and I therefore stopped on my return from Albany, and made a large collection which I take the liberty to send to you with the following account.

The quarry in which the bones are found is on the Albany and Susquehanna railroad, about a quarter of a mile above 'Howe's Cave.' The rock is a limestone of the water-lime group, abounding in vertical fissures. One of these was opened by the workmen in the process of working the quarry. It was several feet in width but was nearly filled by loose fragments which had fallen from the top. Among these fragments are passages worn smooth by long usage and now filled with the bones of the former inhabitants.

The rock forming the sides of the fissure is in some places covered with an incrustation of carbonate of lime an inch or more in thickness. The bones are found in great quantities. Of vertebræ and ribs, I might have collected a bushel in a short time. The man in charge of the work at the quarry told us that they had found deposits of bones that required a man ten minutes to remove with a shovel, and from my own observation I have little reason to doubt his statement. These facts show that the number of individuals that inhabited the cavern must have been enormous.

11. *Mémoires pour servir a l'Histoire Naturelle du Mexico, des Antilles et des Etats-Unis*, par HENRI DE SAUSSURE; III and IV Livr., *Orthopteres—Blattoides.* 280 pp. 4to, with 2 col. pl. Geneva and Paris, 1864–65. (Paris, V. Masson & Fils.).—This important work on the Orthoptera of Mexico and the Antilles is based, as the preface states, mainly on the collections and observations of the author, H. de Saussure, but in part, also, on Mexican specimens received from Mr. Sallé; others from Cuba from F. Poey of Havana, and others from the United States, for comparison, received from Mr. Edward Norton of Connecticut. The volume before us, constituting the third and fourth parts, conclude the work. It is but a small part of the very valuable contributions to Zoology by Mr. de Saussure which have been published as the results of his Mexican explorations.

IV. ASTRONOMY AND METEOROLOGY.

1. *Observations on the Meteors of August last;* by DAVID TROWBRIDGE. (From a letter to the editors, dated Hector, N. Y., lat. 42½° N., long. 0° from Washington, Aug. 11th, 1866.)—The following is a report of my observations of meteoric phenomena at the August period, 1866.

Aug. 9.—I observed from 9 till 9.20 P.M. I saw fourteen meteors, the paths of four converged toward Cygnus, and seven toward Cassiopea, or a little below (to the east, estimated on the meridian). Nearly all left a visible train. Some moved slowly and others rapidly.

Aug. 10.—I observed from 9 till 10.15 P.M. in company, a part of the time, with three other individuals. We saw in all sixty-five meteors; I saw at least fifty of them. The paths of forty-seven of them converged toward Cassiopea. Some of them left long trains, one of which in particular, lasted (by estimation) six or seven seconds. I should judge that the average time of flight did not exceed half a second. Average length of path about 30°. One of them, which did not conform, was from one to one and one-half seconds in its flight. In general those which did not conform moved with less apparent velocity than those which did. Many of them were very small and moved with great rapidity.

Aug. 11.—I observed from 8.15 till 9 P.M. and saw eight meteors, four of which were conformable. It was partly clear a portion of the time. On each evening my attention was confined mainly to the region of the heavens surrounding Cassiopea.

If the rudeness of my observations would allow me to draw any conclusion, I should say that on the 9th the center of the region from which the meteors came was somewhat *below* the *chair* (as seen at the time of observation); on the 10th nearer the chair; and on the 11th *in* the chair.

On the evening of the 26th of July (1866), about 8¼ P.M., a very bright meteor flashed out in Cygnus, and moved from east to west with great rapidity. Its path was about 30° after I saw it. Height above the northern horizon about 50°. Duration of flight from one-half to one second. It left a beautiful train. The head was red and train blue. It was certainly *below* the clouds. It passed between me and some cirro-stratus clouds, so dense as to hide ordinary stars completely. Several others that saw it said it was *below* the clouds.

2. *Observatory of Russia.*—The place of Mr. Kupffer at the head of the Central Observatory of Russia has been filled by the appointment of Prof. Kaemtz of Dorpat.

3. *Mass of Meteoric Iron in Colorado Territory.*—Prof. Henry has transmitted to the Editors a note respecting the discovery of a mass of iron in a deep gulch near Bear Creek, Colorado Territory, about twenty-five or thirty miles from Denver, and 800 or 1000 feet below the top of a steep hill. Mr. James L. Wilson, who describes it in the Daily News published at Denver, Colorado Territory, May 14th, states that it was at first mistaken by himself and Mr. G. R. Morrison who accompanied him and who had seen it before, for the 'blossom' or "iron hat" of a mineral lode. "It is irregular in form, being about twenty-two inches long, nine to ten broad, and fourteen wide. Four of its faces are flat and two rounded. This form indicates it to be a fragment of a much larger mass.

It is magnetic. Its weight is estimated at 500 pounds. The force with which it struck the rocks at the time of its fall had so shattered one end as to enable the discoverers to break off a piece that weighed eleven pounds. Its composition appears to be iron, nickel, cobalt, and copper, unequally distributed in its mass. In one part the nickel and cobalt are largely in excess of the other metals, while in other parts iron forms the chief ingredient. These metals are aggregated and highly crystallized. A coating of the oxyd of iron half an inch thick has taken the place of the shining black crust observed on aërolites when they first reach the earth. The less oxydizable metals, nickel and cobalt, still remain in their metallic state in this coating of iron rust."

It is pretty certain from this not satisfactory description, that this is an example of an iron meteor-mass found where it has fallen, the shattering of the mass and of the adjacent rocks being rarely observed. It was exposed by a freshet which had washed away the loose stones and earth. This is the same mass noticed by Prof. Shepard at page 250 of this issue, who appears to have been in possession of some scales from the concussion which disintegrated the specimen. We have taken steps to obtain more detailed information respecting it.

4. *Reduction of the Observations of Fixed Stars made by Joseph le Paute d'Agelet*, at Paris, in 1783–1785, *with a Catalogue of the corresponding mean places referred to the equinox of* 1800·0; by Benjamin Apthorp Gould. 262 pp., 4to. From the Memoirs of the National Academy of Sciences, Vol. I. Washington, 1866.—This paper, which makes a worthy beginning of the Memoirs of the National Academy of Sciences, opens with some account of the astronomer d'Agelet, by whom the observations were made, a description of his instruments, explanations of the methods of reduction adopted, and remarks on other points of interest, and then proceeds with the tables of reductions. D'Agelet was appointed Professor of Mathematics at the *Ecole Militaire* in 1777. In 1785, after the last of the observations above mentioned were made, he left France as astronomer of LaPeyrouse's expedition around the world, and shared its fate, the vessels having been wrecked in 1788 (as ascertained forty years afterward) on the reefs of Malicollo, one of the New Hebrides, and all on board lost. D'Agelet had been very industrious throughout the course of the expedition, having established an astronomical observatory at each of the ports visited. But his commander did not allow him to send any of his results home, and none of them were saved.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *The American Association.*—The American Association for the Advancement of Science, after a suspension of its meetings for five years in consequence of the war, held its fifteenth meeting at Buffalo, N. Y., commencing on Wednesday, August 15th, and continuing until Tuesday, the 21st.

The officers of the meeting were: Pres. F. A. P. Barnard of Columbia College, *President;* Dr. A. A. Gould of Boston, *Vice President;* Prof. Elias Loomis of Yale College, *General Secretary;* Prof. Joseph Lovering of Harvard College, *Permanent Secretary;* Dr. A. L. Elwyn of Philadelphia, *Treasurer.*

Though there were fewer members in attendance than at some former meetings, owing partly to the shortness of the notice, and partly to pecuniary and other considerations, yet in the number and value of the papers presented, the earnestness and ability of the discussions, and the general harmony of the proceedings, this meeting was throughout eminently successful, and will compare favorably with preceding ones. It left in the minds of the members a strong conviction of the importance of the Association to the science of the country, particularly as a means of stimulating research, and promoting friendly intercourse among scientific men.

Several prominent members, unavoidably absent, expressed their regrets by letter, as well as their abiding interest in the Association. The feeling appeared to be general among the members that, hereafter, the meetings, from year to year, would be fully attended, as before the war.

The success of the meeting was very greatly promoted by the cordial coöperation and generous hospitality of the citizens of Buffalo, who not only were prepared to entertain at their homes all who came, but, by social receptions, excursions on the lake and to Niagara, and many other acts of kindness and appreciation, contributed largely to the enjoyment of the members, as well as to their own reputation for intelligence and public spirit.

The next meeting is to be held at Burlington, Vt., commencing on the 21st of August, 1867. The following officers were appointed for the year ensuing: Prof. J. S. Newberry of N. Y., *President;* Prof. Wolcott Gibbs of Cambridge, *Vice President;* Prof. Joseph Lovering of Cambridge, *Permanent Secretary;* Prof. C. S. Lyman of New Haven, *General Secretary;* Dr. A. L. Elwyn of Philadelphia, *Treasurer.*

The following titles of the papers read are from the newspapers of Buffalo, in which quite full reports were given:

The Spots on the Sun; Prof. E. Loomis.
On the period of Algol; Prof. E. Loomis.
On the path of the meteoric fire-ball of 1860 which passed over Buffalo; Prof. Coffin.
The Dearborn Observatory of Chicago; Prof. T. H. Safford.
Mutual action of electrical currents; E. B. Elliott.
New method of illuminating apparatus for opaque objects under the microscope; Pres. F. A. P. Barnard.
Achromatic registration of meteorological phenomena; G. M. Hough, of Albany.
Map of Magnetic Declination; J. E. Hilgard, U. S. Coast Survey.
Effect of sunshine on fire; Prof. E. N. Horsford.
On the automatic barometer; G. M. Hough.
On the anthistometer; Dr. L. Bradley.
Theory of meteors; D. Kirkwood.
On the Aelloscope; Henry A. Clum.
General meteorological features of the west; Prof. O. N. Stoddard.
On fundamental Star-catalogues; Prof. T. H. Safford of Chicago.
On a new method for the construction of Life and Annuity tables; E. B. Elliott of Boston.
On the statistical systems of certain countries of Europe; E. B. Elliott.
On Decimal weights and measures; B. S. Lyman.
On the galvanic battery; Dr. Bradley.

The Geology of Southern Minnesota; Prof. James Hall.
Structure of the mountains and valleys in Tennessee, Northern Georgia, and Alabama; James Hall.

On the Stromatiporidæ; Prof. A. WINCHELL.
On the Rocks of Kansas; G. C. SWALLOW.
The Laurentian Limestones and their minerals; T. STERRY HUNT.
On the primeval atmosphere; T. STERRY HUNT.
On petroleum; T. STERRY HUNT.
On the internal structure of Athyris, Meristella, and the allied genera; JAS. HALL.
On the structure and mode of growth of the spines on the cardinal area of Chonetes; JAMES HALL.
On Cryophyllite, a new mica; Prof. J. P. COOKE.
On a new chemical nomenclature; S. D. TILLMAN.
On the genesis of auriferous sulphids; H. WURTZ.
The Glaciers of the St. Lawrence; Col. WHITTLESEY of Cleveland.
Glacial epoch in the valley of the Mississippi; Dr. NEWBERRY.
On the drift and its origin; JAMES HALL.
On the drift of the Western and Southern States, and its relations to the Glacier and Iceberg theories; E. W. HILGARD of Mississippi.
Evidences of Glacial action in Southeastern New York; JAMES HYATT of Bengall, N. Y.
On the supposed plasticity of pudding stones; B. S. LYMAN.
On a section of the strata in Northeastern Ohio and Western Virginia; Prof. E. B. ANDREWS.
On the origin of prairies; Dr. J. S. NEWBERRY.

On steam-boiler explosions; Prof. O. N. STODDARD.
On the effects of alum as used in making bread; Prof. E. N. HORSFORD.
On the fruit-producing belt of Michigan; A. WINCHELL.
Proportional dimensions of the human frame; B. A. GOULD.

A eulogy on the late President Hitchcock was delivered by Mrs. A. L. Phelps, and an address on scientific studies as a means of mental discipline, by Prof. J. P. Cooke. The Association passed a resolution in favor of the introduction into common use of the decimal system of weights and measures.

2. *Addition to Article on Method of correcting Monthly Means* (page 154); by E. L. DEFOREST.—In general, if we have any three consecutive monthly means given, and wish to infer from them, as nearly as possible, what the form of the curve must be, our knowledge respecting it comes under two heads. First, it must be a curve of three parameters; for the three given monthly means are sufficient to determine three, and only three, constants in the equation of the curve. Secondly, it ought to be a periodic curve, with an annual period; for we know that at the end of one, two, or any whole number of years, the same monthly means occur over again. Now the parabola satisfies the first of these two conditions, but not the second. The trigonometrical curve satisfies both; so that there is reason for preferring it in all cases.

3. *Flint implements.*—V. CHATEL has recently collected some hundreds of chipped flints, of the age of Stone, in the fields of his estate of Campandré-Valcongrain and the surrounding villages. They were found at the surface, especially after ploughing. Among them there is a saw well toothed, about six centimeters long; a whistle of stone which gave out a very acute sound. On the same estate there are wooded hills which contain several hundreds of Celtic tombs, some of which, of an oval form, are five to seven meters long and project above the surface more than a meter. There are also large blocks of stone, said to be Druidic, which Mr. Chatel regards as ancient altars, and which may belong to the age of Stone.—*Les Mondes*, p. 137, May 24.

4. *Library of works on Earthquakes and Volcanoes of Prof. Alexis Perrey.*—Professor Perrey, of Dijon, has recently offered for sale his very extensive library—probably the best on the two topics of Earthquakes and Volcanoes in Europe. The catalogue of it which has been published shows that it contains 4015 works, including pamphlets. It would be an exceedingly valuable acquisition to any of the larger American libraries, and we hope that it may not fail to be secured to the country. Professor Perrey has been a long and earnest laborer in his favorite departments of science and has spared no pains to make his library a complete one.

OBITUARY.

Prof. JOHN A. PORTER.—John Addison Porter died at New Haven, Conn., on the 25th of August. Prof. Porter was born in Catskill, N. Y., March 15th, 1823, and graduated at Yale College in 1842. Possessed of literary as well as scientific tastes, he was called to fill the post first of tutor, and then of Professor of Rhetoric, in Delaware College, in Newark, N. J., where he resided from 1844 to 1847, when he went to Germany and studied chemistry under Prof. Liebig. Returning in 1850, he first filled the Professorship of Chemistry applied to the Arts in Brown University for two years, when he was called to take the place of Prof. John P. Norton, then recently deceased, as Professor of Analytical and Agricultural Chemistry in Yale College. In 1856 he was transferred to the chair of Organic Chemistry, which position he held until he felt constrained by his declining health to resign it, in 1864.

He then spent a year in Europe, to avail himself of the best medical advice, and returned less than a year ago, apparently greatly benefitted in health. It was soon evident, however, that his disease was not wholly eradicated, and he has at length, after months of intense suffering, during which he exhibited a remarkable degree of fortitude and Christian resignation, gone to his final rest.

In connection with the Sheffield Scientific School, the activity and zeal of Prof. Porter enabled him to do excellent service, both for the institution and the cause of agricultural science throughout the United States. He was chiefly instrumental in originating and conducting the very successful course of Agricultural lectures, which, in 1860, attracted large numbers of persons to New Haven from distant parts of this country. In the reorganization of the School, about the same time, he took an active part, and some important changes were largely due to his forecast and energy.

Prof. Porter was a ready and forcible public speaker, with a clear and flexible voice and a fine personal appearance. His labors in behalf of the Sanitary Commission and in promoting in various ways the cause of his country, were particularly earnest and effective, so long as his failing health permitted him to work. He was ever zealous for truth and justice, and in carrying out his plans of usefulness, exhibited great fertility of invention, and the most unwearied assiduity.

He is the author of a text-book on Chemistry, which has gone through many editions.

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *Geological Survey of Illinois:* A. H. WORTHEN, Director. Volume I, Geology. xvi, and 504 pp. large 8vo, with maps and sections. 1866. Published by the authority of the Legislature of Illinois.—This first volume of the Geological Survey of Illinois, by its able Director, Mr. Worthen, has just been issued, and so near the publication day of these pages that we can at this time barely announce its appearance. The volume is full of matter of great interest to American geology and to science in general, and at the same time it must meet the demands of those at home who have looked to the State Geologist for an exposition of the mineral resources of the State. The various subjects are well treated, and evidently as the result of extended and faithful exploration. The style of publication is every way handsome and generous. Mr. Worthen has had the assistance of Prof. J. D. Whitney in the survey of the lead region; of Prof. Leo Lesquereux in that of the Coal formation and the subject of prairies; and of Mr. Henry Engelmann in the general field work and chemistry. The volume will be followed by another on the Paleontological part of the survey, in the preparation of which Mr. Worthen has been aided by Mr. F. B. Meek, Dr. J. S. Newberry and Prof. Lesquereux.

This notice of the Geological volume is but an introduction to an abstract of its contents which we propose to give in another number.

2. *Reliquiæ Aquitanicæ*, being Contributions to the Archæology and Palæontology of Périgord and the adjoining Provinces of Southern France; by EDOUARD LARTET and HENRY CHRISTY. Parts I and II, Dec. 1865 and March 1866, each 24 pages 4to, with 6 lithographic plates. London. (H. Bailliere. Each part 3*s.* 6.)—The *Reliquiæ Aquitanicæ*, or Aquitanian remains, are flint implements and the bones associated with them in Southern France (the region being part of the Aquitania of the Romans); and the work upon them bearing the above title treats of these implements and bones, and the bearing of the facts relating to them on European Archæology. As the preface states, the work was projected by Mr. Christy; and its first sheets were already in the press, and some of the plates engraved, when he died after a brief illness brought on by over-exertion in a visit to the Belgian bone-caves. The principal labor of preparing the work was thus thrown upon his fellow-worker, Mr. Lartet, who is now aided by Mr. P. l'Haridon of France, and Messrs. John Evans, A. W. Franks, W. Tipping and Prof. T. Rupert Jones, the last-mentioned taking the editorial duties. The work is issued in elegant style, both as to text and plates, and treats of one of the most interesting subjects in modern science. The figures give admirable views of the flint arrow-heads, chippings, etc.; of the bones, part of them carved or etched with figures of different animals upon them; and of the arrow heads, harpoon heads, etc., made of reindeer's horn. It is to be completed in about 20 parts of 24 pages and 6 plates each.

Baird's Review of American Birds.—We have received sheets 21 to 28 inclusive (pp. 321–450) of Professor Baird's work.

Geological Map of England and Wales, by Prof. A. C. RAMSAY, F.R.S. 3d edition. London. [Stanford.]

On the Anatomy of Vertebrates; vol. II. Birds and Mammals; by R. OWEN, F.R.S. London. [Longmans.]

The British Pleistocene Mammalia; by W. Boyd Dawkins, Esq., M.A., F.G.S., and W. Ayshford Sanford, F.G.S. Part 1. Published by the Palæontological Society.

The Anthropological Treatises of Johann Friedrich Blumenbach, with Memoirs of him by Marx and Flourens, etc., translated by T. Bendysche, M.A. Published for the Anthropological Society. [Longmans.]

Histoire des Crustacés fossiles; par E. Blanchard. The first volume of this work on Fossil Crustacea was presented by the author to the Academy of Sciences in April last.

Animaux fossiles et Géologie de l'Attique, d'après les recherches faites par M. Albert Gaudry. Paris. (E. Lévy.)—The 13th (last) part of this important work has been issued. The whole makes a volume in small folio of 323 pages, with 52 lithographic plates.

Clavis der Silicate, von Dr. Leop. Heinrich Fischer. 114 pp. 4to. Leipzig 1864. (W. Engelmann.)—Consists of tables for the determination of the mineral silicates.

Jahresbericht des Vereins für Erdkunde zu Dresden. Dresden, 1865. Erster, 30 pp. 8vo; Zweiter, 57 pp. 8vo, with also a paper of 24 pp. entitled Der Chaldäer Seleukos; eine kritische Untersuchung aus der Geschichte der Geographie, von Dr. Sophus Ruge.

Archiv für Anthropologie. Zeitschrift für Naturgeschichte und Urgeschichte des Menschen; by C. E. v. Baer of St. Petersburgh, E. Desor of Neufchatel, A. Ecker of Freiberg, W. His of Basel, L. Lindenschmit of Mainz, G. Lucae of Frankfurt a. M., L. Rütimeyer of Basel, H. Schaaffhaussen of Bonn, C. Vogt of Geneva and H. Welcker of Halle, under the special direction of A. Ecker and L. Lindenschmit. Appears irregularly in parts, 3 of which make a volume. Published by Friederich Vieweg & Sohn, at Braunschweig.

Proceedings Acad. Sci. Philadelphia, No. 2. April, May, 1866.—Page 101, History of the "small black erratic Ant"; *G. Lincecum.*—p. 110, Notes on some members of the Feldspar family; *I. Lea.*—p. 113, On Chætetes and some related genera, with descriptions of species; *C. Rominger.*—p. 123, 4th Contribution to the Herpetology of Tropical America; *E. D. Cope.*—p. 133, On 5 n. sp. of Unio, and 2 of Lithasia; *I. Lea.*—p. 134, Critical review of the Procellariidæ, Parts IV and V, with a general supplement; *E. Coues.*—p. 197, On the cranial forms of the American Aborigines; *J. A. Meigs.*

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